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PHILOSOPHICAL MAGAZINE
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JULY 1822.

I. *Further Observations on Dr.* READE'S *Papers on Refraction.* *By Mr.* CHARLES STARK, *R.N.*

To Dr. Tilloch.

SIR, — YOUR correspondent Dr. Reade having, in the Number for March, favoured us with another paper illustrative of his new System of Optics, I beg to offer a few more observations on that interesting subject.

From the Doctor's former paper, it appeared that he felt quite convinced of having demonstrated that no such thing existed as the refraction of light; and that what philosophers had hitherto attempted to explain by having recourse to that principle, might be accounted for much more simply on the principle of reflection. It appears too, from his last communication, that his opinions on this subject still remain unshaken,—that he feels quite convinced of having completely subverted the Newtonian system of optics: besides, he tells us that his opinions are daily gaining ground, and received by men of the first eminence.

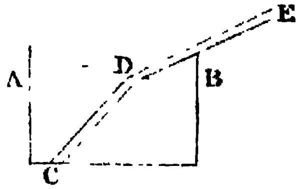
In entering into any critical examination of Dr. R.'s papers, I have no wish to throw a shade over the bright prospects which he must, no doubt, be enjoying of seeing that his name must stand pre-eminent in the future annals of philosophical discovery; nor am I actuated by any "angry" motives, as he has unfortunately been led to suppose; but with every feeling of respect to Dr. Reade as a gentleman and a man of science, I shall here take the liberty to point out to him and his followers, the absolute necessity of admitting the law of refraction as well as reflection into the science of optics.

Dr. Reade, at the commencement of his last paper, again gives

gives a detail of the experiment of placing a piece of money at the bottom of a tumbler partly filled with water; and from his observing that the piece was seen when the tumbler was held below, on a level with, and above the eye, he concludes that in each case it is seen by a reflected image formed perpendicularly over it on the surface of the water.

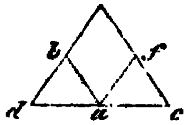
Before entering into any examination of the weight of Dr. R.'s arguments, I shall here mention two experiments which I think are of themselves quite sufficient to set at rest the whole of his reasoning on the subject.

Exp. 1.—To preclude the possibility of the surface of the water in the tumbler becoming a reflecting surface, I covered it over with a circular piece of dry flannel, with a small semicircular hole cut out of its edge. On holding the tumbler below the level of the eye, the half-crown was seen through the opening in the edge, in the same manner as before it was covered; but on holding it above the eye no image whatever could be seen. I would here ask Dr. R., how it happens that the reflected image is destroyed in the one case and not in the other.



Exp. 2.—Having formed a small tube of pasteboard with an angular bend in it at D, as in the figure, so that nothing could be seen through it in the open air, I placed the end C on the bottom of the tumbler A B, the part C D making an oblique angle with the surface of the water, and having the point D exactly in that surface. By holding a candle under the point C, and looking through the tube from the other end E, the bottom of the tumbler was seen quite distinctly, the tube at the same time appearing nearly straight. When a straight tube was used and held in the same direction C D, nothing whatever could be seen through it. From this experiment I may also draw Dr. Reade's weighty inference, that "*to see is to believe;*" but that to see an object through a bended tube in the above manner, is to believe that the rays of light in their progress from the object to the eye follow the direction and bend of that tube; or, in other words, they are refracted in passing from water into air.

What appears to me to have led Dr. R. astray in most of his reasonings is the singular opinion he seems to entertain, (although he has not expressly mentioned it,) that the rays of light do not proceed in all directions from every point of an object, but that they all go on in one particular direction parallel to each other. For example, in his first experiment, he says, "Let us examine this experiment according to the received laws laid down in every elementary treatise on optics; and I contend that no refraction or bending of the rays can possibly take place at *d* (see his figure), for the rays *cd* enter the air perpendicular to the plane surface of the water: consequently they must pass on without any refraction." Here the Doctor evidently supposes that only a cylinder of rays proceeds from the half-crown perpendicular to the surface of the water. A number of his other arguments involve also this gratuitous assumption. "When the eye," says he, "is placed immediately over the half-crown looking down into the water, we see the image, not the piece of money, one-fourth nearer to the eye: here there can be no refraction, as the rays coming to the eye must be at right angles to the surface of the water; here there is no angle of incidence; no angle of refraction; no ratio of 3 to 4." Here the Doctor's reasoning is no doubt conclusive, if we admit him his own principle which he has here again assumed as an axiom; for in this case the half-crown could not evidently be all seen at once, unless the pupil of the eye were at least as large as the half-crown itself; and taking the other parts of the eye proportional to this size of the pupil, we [illegible] safely conclude that no one since the creation has been [illegible] with such organs of vision. In the experiment which he gives with the prism, his arguments also hinge on the same principle. "Having," says he, "placed a sovereign under the plane of an equilateral prism, I found that two reflected and not refracted images were formed in each plane, as represented in the following figure.



a the sovereign placed under the plane *dc* of an equilateral prism, forms an image at *a*; which image sends images to *b* and *f*. According to the present theory, two images could not possibly be formed by refraction at *b* and *f*; for *a* being at right angles to the plane *dc*, the rays should suffer no refraction, but proceed on to the vertex." Here the Doctor would have much obliged his mathematical readers, had he informed them what he meant by a point being at right angles to a plane; but from his usual mode of reasoning, we may suppose that he means *the cylinder of rays from the sovereign* rises at right angles to *dc*. However, that they are not reflected images may be made evident by turning the prism round on one of its angles *d* or *c*, for then the sovereign will appear quite distorted and tinged with the prismatic colours; whereas it is well known that any object seen by reflection from a plane surface never appears to be altered in shape, but always preserves its natural form.

Dr. Reade seems surprised also that Sir Isaac Newton was not acquainted with the formation of two spectra when the rays of light were made to fall perpendicularly on one of its plane sides; but I should suppose that he considered this as a self-evident corollary of the general principle of refraction. Indeed, if Newton had mentioned this as a particular discovery, he might as well have told us that when two opaque bodies were interposed between the sun and a wall, there were also formed two shadows. Dr. R. says that "mathematicians are here obliged to relinquish one of their favourite laws, that rays striking at right angles to plane surfaces suffer no refraction;" but he will here be pleased to recollect, that when rays fall at right angles to one of the faces, they must strike either of the other faces obliquely, and consequently be refracted at their emergence.

I should consider it an idle task to proceed any further in the refutation of doctrines which do not carry along with them any thing like demonstrative evidence, but hinge entirely on the author's own *ipse dixit;* my principal object being only to show the inconsistencies which resu[illegible] rejection of the law of refraction.

The Doctor has requested me [illegible]per on Vision, published in a former Number of [illegible]ne; but I suppose he must mean that which he published some time ago in "The Annals of Philosophy," which I have also read; but consider it quite foreign to the matter in question, whether the ideas of visible objects be conveyed to the mind by retinal or corneal images. I am, sir,

Your most obedient servant,

CHARLES STARK.

His Majesty's Ship Queen Charlotte,
Portsmouth Harbour, May 26, 1822.

II. *An Account of the Repeating Circle, and of the Altitude and Azimuth Instrument; describing their different Constructions, the Manner of performing their principal Adjustments, and how to make Observations with them; together with a Comparison of their respective Advantages.* By EDWARD TROUGHTON, *Esq. F.R.S., and Member of the American Philosophical Society*[*].

Of all astronomical instruments, those fixed in national observatories must be considered of the first importance to science; and in a commercial country, like our own, perhaps those subservient to nautical astronomy ought to be regarded as the next point in of utility. Those which I would call the third class are numerous; they are such as are used in the small observatories of the amateur, to which they are in general equally adapted, as to the service of the gentleman who may travel to foreign parts. Of those, the two I have named in the title, are the most approved of for these purposes; and to draw up a comparison of their respective constructions and merits, is what I have chosen for the subject of this communication. Were I able to treat it as it deserves, I should entertain no doubt of its coming within the views of this Society, nor of its usefulness; particularly in assisting those, who may not already have become acquainted with the different kinds of instruments, in the selection of such as may be best suited to their purposes.

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[*] From the Memoirs of the Astronomical Society of London.

The repeating circle, till within these few years, has been very little used in this country, and in truth its merit but ill-appreciated; facts however are not wanting, although dispersed and insulated, sufficient to remove all prejudice; particularly experiments recently made, with a small instrument of this kind, at the principal statio[illegible] our grand national survey. On the continent of Euro[illegible]re the art of graduation is not so successfully cultivated [illegible] with us, an instrument which of all others depends th[illegible]pon accuracy of division, could hardly escape being t[illegible]ch commended: be this as it may, observations lately made on the other side of the British channel, simultaneously with those used in the survey mentioned above, have I believe given the best informed of all parties a more correct idea of what may be expected from this instrument.

The altitude and azimuth instrument has I think been almost exclusively made in this country: many of them have been sent abroad, but from their not having been used in great national operations, the advantage of them has seldom been made known to the world. Nearly the same may be said of those which remain at home: for although some of them have been much and skilfully used, yet owing to their having been only in the hands of private individuals, who had no common medium of communication, the labours of those who possessed them have hitherto been almost lost to astronomy. From this general remark I must however except the observations of the 36 brightest fixed stars, which Mr. Pond made at Westbury with a 30-inch circle of this kind, and which appeared in the Phil. Trans. for 1806. This indeed was the first thing (notwithstanding some doubts and surmises from abroad) that unequivocally demonstrated a change of figure in the Greenwich quadrant, and subsequently led to the procuration of new instruments for our national establishment.

The repeating circle has by no means failed for want of publicity; on the continent, astronomers and others have written a great deal about it, and the results of thousands of observations have been published; the greater part of which were made on *Polaris*; a star, to which, on account of its slow motion, this instrument is peculiarly adapted. Although the altitude and azimuth instrument, as a portable one, was produced about the year 1792, we find no description of it in print, until the article CIRCLE appeared in Rees's and Brewster's Encyclopedias*; the latter of which is referred to for the use of those who may wish to see a more detailed account of both the instruments under consideration, than can be given in the following brief descriptions.

Description of the Repeating Circle.

The lowest part of this instrument is a strong tripod, having at its extremities three steady foot-screws: one of which, at least, should stand upon a well known apparatus, for the purpose of supplying a slower and finer motion to the upper part, than can be given by the screw itself. This apparatus should support that particular foot which during observation is directed to the meridian, or is opposite to the object observed. In the centre of the tripod is fixed a strong vertical axis, of a height sufficient for allowing head-room for observing conveniently when the telescope is pointed towards the zenith. A pillar of the same height with the axis, is nicely fitted at both ends upon the latter, and both together, when the axis is vertical, produce a steady azimuthal motion. To the lower end of the pillar is fixed an azimuth circle: and to the higher end, a cross piece; on the two extremities of which stand, about five inches apart, two upright bars for supporting a cross axis, to which the principal circle by its centre-work is attached, and round which axis the circle may be turned into any position from one side of the pillar to the other. A semicircle is fastened to one end of the cross axis, which, together with a clamp attached to one of the upright bars, affords the means of securing the circle in any position. The principal circle, or that of repetition, has (affixed to the middle of its plane, and opposite to the one divided) centre-work, the length of which projected to about two-thirds of the diameter of the circle; the outer part of which, being perforated from end to end, becomes the socket for an axis of the same length, to which the index of the circle and telescope is attached in front. The index has four branches placed at half right angles to the telescope, each of which subdivides the divisions of the circle into spaces of 10″. To the middle of the cross axis is fixed a socket, which receives about two-thirds of the length of the centre-work: and the exterior surface of the remaining third of that work becomes the axis for another telescope and a level to revolve contiguous to the back of the circle. This is a complicated matter, difficult to be described or understood without a figure: it will however be sufficient, if it is conceived that there are three concentric motions in planes parallel to that of the circle: namely, a general one within the socket of the cross axis, which carries round together, the circle, level and two telescopes: another, by which, upon the exterior part of the centre-work the level and back telescope revolve; and a third, that gives motion to the fore telescope and the verniers, so as to make them advance upon the circle, which is produced from the interior axis. These motions are independent of each other, and are all furnished with clamping and tangent screws. A counterpoise is placed upon the exterior end of the centre-work, which, by balancing the circle, telescopes, and level, keeps them stationary in any position. The greatest part of these instruments, which have been constructed in London, have the back telescope on one side of the axis, and the level parallel to it on the other side, which latter, being made heavier than would be otherwise required, becomes a counterpoise for the former, a thing not attended to in the earlier constructions of the repeating circle. The azimuth circle of this instrument, only just named above, was in the first construction small, and of no other use than to point out roughly when the upper circle had been turned half round; but, in most of those made in London, to that circle has been given the same radius, and the same attention paid to its execution as to the upper one. In the best construction of this part the circle is attached to the tripod, and three indices fastened to the vertical pillar revolve round it: thus may a horizontal angle be taken on three equidistant parts of the circle, and, what is of equal importance, by simply reversing the position of the telescope and turning half round in azimuth, a similar observation may be made, in which the readings will fall at 60° distance from the former ones. By this double operation simple errors of division may be considered as very much diminished, each sight having been read off on six places; and in both parts then the operation the error arising from eccentricity is, as to any sensible quantity, totally done away.

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* It is true that the late Rev. Francis Wollaston, in the Phil. Trans. for 1793, gave a description of a two-feet circle which had an azimuth. That instrument, however, was solely designed for a meridian one, and was in fact quite unfit for any other purpose. The same gentleman, in the Appendix to his *Fasciculus*, points out the best means of using an altitude and azimuth circle (properly so called), but without giving any description. The Westbury circle described in the Phil. Trans. for 1806, although well constructed for observing azimuths, was not designed for taking transits, and besides was not a portable instrument.

Description of the Altitude and Azimuth Instrument.

The lower part of this instrument, like the other, consists of a tripod and feet-screws; which latter, being a recent contrivance, and hitherto undescribed, may in this place deserve particular notice. Each of the three screws is double; that is, a screw within a screw: the exterior one, as usual, has its female in the end of the tripod, and the female of the interior screw is within the exterior. The interior one is longer than the other, its flat end rests on a small cup on the top of the support, and its milled head is a little above the other. Now by this arrangement we gain three distinct motions: for, by turning both screws together, an effect is produced equal to the natural range of the exterior screw: by turning the interior one alone, the effect produced is what is due to this screw: and by turning the exterior one alone (which may be done, because the friction of the interior screw in the cup is greater than that which exists between the two screws) an effect is produced, equal to the difference of the ranges of the two screws. Thus, were the exterior one to have 30 turns in an inch, and the interior 40, the effect last described will be exactly equal to what would be produced by a simple screw of 120 threads in an inch. This is an improvement applicable to all instruments that are supported on screws, and of course to the repeating circle. A few of the last made in London possess this advantage. The vertical axis of the altitude and azimuth instrument is fixed in the centre of the tripod, of a length equal to about the radius of the circle. At the lower end is centred upon it the azimuth circle, in close contact with the tripod; to the three branches of which it is fastened, but in such a manner as to admit of a circular motion of about 3, which motion is governed altogether by a slow moving screw. The intention of this motion is, in geodetic operations, to bring the zero of the circle to the point of commencement; and, in astronomy, to place that point exactly in the meridian. In an instrument for my own use, however, I could dispense with this adjustment, because I know that it is easier and more accurate to *read off* than it is to *set*: and from what point I begin to reckon, is a thing quite indifferent to me. Just above the circle the axis is embraced by a cone, which is also well centred upon the upper end. To the lower end of the cone is fixed an entire circular plate, formed in the strongest manner; which not only bears the two or three microscopes that read and subdivide the divisions of the circle, but also supports the whole of the upper works. On opposite sides of the cone, and distant from it about half the radius of the plate, are erected two columns, of a height to support a transit axis, so as to allow the telescope to pass the upper end of the vertical axis, when it is pointed towards the zenith. The transit axis is one-third longer than the distance between the columns, upon which *out-riggers* are placed, having Y's or angles on their extremities, that support the axis: and each of the angles is acted upon by an adjusting screw, not only for making the transit axis horizontal, but also for placing the centre of the circle of the same height with the horizontal microscopes. The horizontal axis is crossed, as in an ordinary transit, by a telescope; the length of which exceeds the diameter of the circle by about one-third. The circle framed upon the axis is double: the two parts being placed at a distance from each other to allow the telescope a lodging between them: and they are connected with each other by pillars inserted perpendicularly between them. The front portion of the circle (or that which bears the division) is of a less radius than the other by about one-eighth part of an inch: the longer radius of the portion behind is what is required for the clamp and screw for slow motion to act upon, while the shorter radius of the one in front keeps it clear of that apparatus when the axis is reversed for collimation. Many of these instruments have been constructed with vernier readings; but as I consider those by the microscopic micrometer preferable, I shall confine this description to the latter. My preference to one of those excellent contrivances for minute subdivision is mainly grounded on the circumstance that, in the employment of the more ancient method, the indices rub against the divisions which they subdivide: whereas in the modern, which is detached, the motion is free and unembarrassed.

In the description of the repeating circle, the advantage of three readings was stated: but that contrivance originated with the instrument I am now describing: and if it be a real improvement (which I believe no one will doubt), the repeating circle owes the advantage solely to the latter. Three readings are not only better than two, but also better than four: for, with four, when the objects are in the horizon, or near it, on reversion the opposite indices only change places: a circumstance clearly in favour of the odd number. But, in astronomy, where the upper circle is chiefly concerned, the same advantage does not occur: for, at the zenith, on reversion, the telescope changes place in azimuth only: therefore, as the indices have no change of place, more readings than two could be of no use. It is true that in proceeding downwards when the gradually come to the horizon, where the same effect, that was stated respecting the azimuthal angle, takes place: but here the uncertainty of refraction destroys all confidence. I may also state that three readings to a vertical circle cannot be all equally well illuminated in the night time; nor at any time are they to be read with equal convenience, as is the case where two readings are placed horizontally. However, as microscopic readings are expensive, and as astronomy is generally the chief object of those who procure this instrument, two microscopes to each circle may be sufficient. But were I to have for my own use an instrument of this kind with verniers, the lower circle should have three, and the upper one four.

When the vertical circle has two readings, and these microscopic, they are affixed to the ends of two horizontal tubes fastened to one of the columns; which also support a good hanging spirit level. Another level of the best quality is occasionally applied to the pivots of the transit axis, in order, independently of every thing else, to verify its horizontality.

Adjustment of the Repeating Circle.

The vertical axis is made perpendicular by means of the feet-screws of the tripod, and the spirit level, in the same manner as is required for other intraments; an operation so easy and well known, that to mention it is all that seems necessary. To adjust the collimation of the telescope parallel to the plane of the circle, an object should be chosen as nearly in the horizon as can be estimated: the middle wire of the telescope under adjustment, being correctly pointed to the object, what is shown on the indices of the azimuth circle must be carefully noted. Reverse the telescope both vertically and horizontally, bisecting again the same object with the same wire, and again read off what the indices give. Take of these readings the mean, or middle point, and set with great care the indices so as to show that mean. Now, by the screws, which act upon the wire-plate, move the wire so as to make it bisect the object: this being well done, the other telescope, to be adjusted, wants only its vertical wire moved in the same manner till it bisects the object. The above, however, is true only when the object is very distant; for, as both telescopes are eccentric, as respects the vertical axis, and unequally so, it becomes necessary, when no remote object can be seen, to put up marks, say two circles, the radii of which are equal to the eccentricities of the respective telescopes. The next essential adjustment, is to place the plane of the upper circle vertical; or its axis horizontal. The best practical method of doing this, and which is quite equal to the purpose, is to look with the front telescope at any elevated object, whether remote or near, and having made the middle vertical wire bisect it, look at the same object when reflected from the surface of a fluid. If the wire does not cut the reflected image, the circle must be turned round the cross axis, to bring the wire as nearly as can be estimated half way towards that image; now by turning the intrument in azimuth, make the bisection, then elevate the telescope to the object, and if the bisection is not perfect, the operation of estimating and turning in azimuth must be repeated. A level, which is placed parallel to the axis of the circle, must now be adjusted so that the bubble may stand in the middle of its tube; which afterwards becomes the index for the vertical position of the circle. Another adjustment, which is not however of so much importance as either of the former, is to make the cross axis at right angles to the vertical one; which is indeed the business of the maker. If, when the vertical axis is adjusted, he brings the upper circle horizontal by means of a pocket-level, which is to be placed upon the face of the circle at right angles to this axis, then, by placing the level parallel to the axis, he will see which of the supports wants to be shortened by the file.

Adjustments of the Altitude and Azimuth Instrument.

The axis of azimuth is rendered vertical by means of the level and feet-screws; exactly in the manner that was required in the other instrument; and it may be stated that either or both of the levels belonging to it may be used for this purpose. That adjustment, which answers to the second for the repeating circle, or setting the line of collimation perpendicular to the axis, is no other than the usual way practised for doing the same thing in a plain transit: namely, by moving in azimuth bring the middle vertical wire to any object, then reverse the horizontal axis end for end upon its supports, and if in this position the wire does not cut the same object, alter one half the error by turning in azimuth, and the other by means of the screws which act upon the wire-plate. The transit axis is brought to the horizontal position simply by placing the level upon the pivots of the axis, and observing if the air-bulb changes its place on turning the level end for end. If it does, nothing more is wanted to effect the adjustment, than with the screw below either of the pivots, to bring the bubble, according to the indication of the divided ivory scales, just half way towards the place which it occupied in the first position.

Both the circles under consideration require many more adjustments: but as those belong to the minor parts, and are common to many instruments, even to enumerate them in a paper like this, could hardly answer any useful purpose.

Manner of using the Repeating Circle.

In geodetical observations this instrument gives the angular distance between two observed objects, whatever be their elevation above or their depression below the horizon. The horizontal angle is always the thing wanted; to obtain which, it is necessary to find by observation how the objects are situated respecting the horizon; these give the requisite data for trigonometrical computation. Previous however to this, the observed angle itself has to be corrected for the eccentricity of the telescopes; which correction varies according to the quantity of eccentricity, and the measured, or estimated, distance of the observed objects. To place the plane of the repeating circle parallel to the line that joins two objects, the angular distance of which was to be observed, had been no easy task, until about thirty years ago from my little gazebo I attempted to take the angular distance of two spires. Their distance was by no means my object; it was simply to acquire the habit of observing by repetition, and putting to trial an instrument that I thought well of. After having made three attempts, without effect, to obtain the thing wanted, and a fourth placing me still further from the point, I quitted my instrument, disgusted at my own unskilfulness, and retired to consider whether the instrument had not within itself some principle from which a precise rule might be made out. This inquiry proved successful, for I saw that by pointing one foot of the tripod, the cross axis and the back telescope towards one of the objects, the fore telescope by turning round the cross axis and by its own proper motion might be brought to the other object without altering the angular direction of the back telescope. The rule is this. Set one foot of the tripod as nearly as you can guess in a line with that object of the two, which you judge to have the least elevation or depression; and with the plane of the circle vertical, and the back telescope horizontal (both to the exactness of two or three minutes), bring the back telescope to the object, partly by turning in azimuth, and partly by turning or propping the foot-screw. Next turn the circle round on the cross axis, until it seems to the eye to occupy the proper position; then a second time bring the back telescope to the object by the foot-screw, and turning in azimuth; lastly, complete the operation by bringing the upper telescope to the other object by its own proper motion in conjunction with that of turning round the cross axis. The above operation being performed, (which it is necessary to repeat at every angle that is taken, even at the same station,) the business of observing by repetition may be commenced as follows. Set the fore telescope to zero, as is usual; or what is better, as was said before, read off what the indices (being clamped) happen to show; and, by turning round by the general motion, place the intersection of the middle wires exactly on the object to the left. Then, by its own motion in the same manner set the back telescope to the object on the right; and examine if the angle between the objects be accurately comprehended between the two telescopes. Now by the general motion, without touching any thing else, move the back telescope until its wires coincide with the object on the left. To complete the first operation unclamp the fore telescope, and carry it round to the object to the right; when its indices will have advanced upon the graduated limb through an arc equal to double the angular distance of the objects. To read off this double result would be rather detrimental than useful; instead of which, with the fore telescope fixed at this position, the three steps of another operation, as described above, should be taken in order to obtain a second double result. A third, fourth, &c. course of operations must succeed, until it is judged that sufficient has been done to produce the accuracy required. At last the indices must be read, and the total number of degrees, minutes and seconds, that have been passed over by the indices, taken and divided by double the number of operations; when the simple angle between the objects will appear. If all the results had been read, the intermediate errors of division would have come into the account, and produced an effect that has been avoided by the process described: for, except at the beginning and end, the observations were carried on as if there had been no divisions. It is in this solely, that this instrument claims an advantage over others, and justly; for they have a beginning and end to every double result; but this, as far as graduation is concerned, has only a beginning and an end to a whole course of observations. In geodesy, the levels are of no use, except in the operation of bringing the plane of the circle into that of the two objects; and it may be observed here, that in astronomy the back telescope is altogether unnecessary.

To observe zenith distances of the heavenly bodies by repetition, is a process so similar to what has been described, that a shorter course may be taken to explain it. The instrument being adjusted, and the indices set or read, by the general motion (the level being horizontal) bisect the star, and examine that both are correct at the same time: now turn the instrument half round in azimuth, correcting with the foot-screw the position of the level if required, and move the telescope by its own proper motion to the star again, which will cause the indices to pass over an arc equal to twice the zenith distance. Again turn the instrument half round in azimuth, and with the telescope fixed in the last position, by the general motion again bisect the star, and again by its proper motion make the level horizontal: now turn half round in azimuth, correct the position of the level as before, and in order to come at another double zenith distance, carry round the telescope to the star again. This process having been continued until enough has been done, the total arc passed over by the indices, divided by double the number of complete operations, gives the zenith distance of the star. There is indeed another way of observing by repetition with this instrument. For the same effect will be produced, if, instead of turning half round in azimuth, the circle be turned to the other side of the pillar, on the motion of the cross axis. But, in this case, there must either be a stop to prop the circle on the other side when its plane is vertical, or else the level must be a hanging one, which will give the circle its vertical position whether it is above or below the axis. It would be altogether unnecessary to describe the process of repetition in this case; for, except in what has just been stated, it differs not from the former one. A nominal difference indeed takes place; for the former method proceeded by stops of double zenith distance, and this proceeds by stops of double altitude.

[To be continued.]

III. *On the Hypothesis of Gaseous Repulsion.* By JOHN HERAPATH, *Esq.*

Cranford, London, July 4, 1822.

"BUT whether," says Sir Isaac Newton, after having investigated the laws of a supposed repulsion between the particles of aëriform bodies, "elastic fluids do really consist of particles mutually flying one another, is a *physical question.* I have mathematically demonstrated the property of fluids having such particles, that hence *philosophers may take occasion to discuss that question.*"—*Principia*, Book ii. Prop. 23. Scholium.

Notwithstanding this unqualified declaration of Newton himself to the contrary, some philosophers strangely assert, that he has demonstrated the existence of a repulsive property in the particles of gaseous bodies. Convinced of the justness of most of his observations, from the failure of my attempts in a different course in the early part of my pursuits; and satisfied that it is to a steady prosecution of his ideas unmixed with those of others, that I owe whatever success I may have met with, few individuals would be less disposed than I should to controvert, on light grounds, the views, that illustrious philosopher supposes he has proved. So far however is Newton from conceiving he has established repulsion, that he even calls in question the existence of attraction; a property as much the more plausible, as it appears to be more common. "And to show," says he, "that I do not take gravity to be an essential property of bodies, I have added one question concerning its cause," &c.—*Advertisement the second to his Optics.*

After such explicit statements as these, it is somewhat extraordinary that the authority of Newton should be advanced in confirmation of gaseous repulsion. That great philosopher, whatever use he may have made of the hypothesis of repulsion for want of a better, could never, with his views of the nature of heat, as might be easily shown, have believed repulsion to be the cause of gaseous elasticity. Hence I apprehend arises his cautious manner of proposing it; and his wish to precede confidence by further inquiry. But so unaccountably anxious are men for the authority of Newton to sanction their peculiar notions; and so safe do they think their views if a passage from him can be wrested to their support, that I have not only seen him quoted in undisputed corroboration of what he merely favoured, but he has actually been made to sanction that to which he is decidedly and notoriously hostile. Thus, it is not only attraction and repulsion, on the hypothesis of which he has made such brilliant discoveries, he is erroneously forced to have proved, but he has been made to support caloric, the opposite hypothesis to that which he admits. He has, if I recollect rightly, been quoted by Dr. Young, to favour the propagation of light by pressure, which it is well known he has indisputably disproved; and I am informed he has even been put at the head of atheists, though one of the most pious and virtuous men that ever existed!

I have thought it necessary to premise this much, lest I should again be unfairly and falsely charged, as I have already been, with temerarious opposition to Newton; to whose views, I repeat, I think it no derogation to acknowledge, if my labours have been successful, I am chiefly indebted.

The density being proportional to the compression, the phænomena of repelling forces to which Newton has arrived, are, 'that the centrifugal forces reach to and terminate in the next particles, or at most are diffused but a little further, and that the intensity of repulsion is reciprocally as the distance.' Any observations on the singular whim nature must display in operating by forces so limited as to terminate in the nearest particles, however much the air may be condensed, and yet so indefinitely extensive as to reach them with the same limitation

tion and unaffected vigour however much the air may be expanded, would perhaps be thought hypercritical; especially as we have some analogy to it in the action of the magnet. It would certainly be no difficult matter to show, that this pretended analogy will by no means bear out the probability of the hypothesis. Objections however of this kind I shall not stop to advance. My object will be to endeavour to prove that this hypothesis, which Newton has found to agree with one property of airs, is not consistent with other phænomena.

Newton thought it probable that, "as in algebra where affirmative quantities vanish and cease there negative ones begin. so in mechanics where attraction ceaseth there a repulsive virtue ought to succeed." In other words, Newton conceived that there are distances within which particles may attract each other, but without which they repel. No one certainly will deny, but that repulsion, physically speaking, is equally as probable as attraction; and that, if the one exists, no reason appears why the other should not. If, however, a mere separation of the component parts of a body, to a greater distance from each other, be sufficient to change that mutually attractive force which they are supposed to have when nearly in contact into repulsion, then instead of distant bodies attracting one another as they are found to do, they ought to repel, in consequence of the antipathy which the distance has occasioned in the particles of one body for those of the other. Particles of any one body likewise, which are within their common sphere of attraction, should mutually attract each other, and those which are without that sphere should reciprocally repel; so that by this theory a body would be kept entire by the excess of the attraction of the nearer above the repulsion of the more remote particles. Hence, if a part of the body be removed to a greater distance from its surface than the sphere of attraction of the nearest superficial particles extends, it would no longer be attracted, or have a tendency to approach the body of which it formed a part, but would endeavour to recede from it as far as possible; which is contrary to experience.

If, to avoid this difficulty, it be urged that the repelling force exists only when the elementary parts of a body are singly separated to greater distances than their individual spheres of attraction extend; and that in other cases the adjacent particles attract not merely each other, but also those particles that are without their sphere of attraction, then it will follow, that if there be two clusters of particles A and B, and the particles of A, first supposed in their sphere of repulsion, approach each other whilst the clusters themselves retain the same distance, the repulsive force of the particles of the cluster A on those of the cluster B, will either continually diminish, become nothing, and then become attractive, or it will increase to a maximum, and then suddenly be converted into attraction. Either case will render the intensity of the mutual action of the two clusters not under the control of their distance, but of the distance of the parts of the cluster A from one another; a conclusion openly at variance with facts.

Some philosophers will perhaps observe, that they do not imagine the attractive force of the particles of matter destroyed and converted into a repulsive by the intervention of greater space; they allow that the attraction between the particles still exists, but that it is overcome by the action of a discrete fluid which they call caloric, and which they push into the pores of bodies and put round the particles of matter in the form of atmospheres. The particles of this fluid they suppose mutually repel each other, while they attract and are attracted by the particles of other matter. If any of this fluid come in contact with a body, it will, we are told, in consequence of these two properties, diffuse and extend itself throughout the whole of the body; and, by the natural antipathy which its particles have towards one another, will endeavour to make the particles to which it adheres recede and separate. Should there be enough of this fluid to overpower the mutual attraction of the parts of the body, these parts will separate into the form of gas, and stand at the greatest distance from each other which the space will allow them. By these views it appears that the attraction of the particles is not destroyed, nor even virtually weakened, but merely exceeded by the repulsion in the calorific atmospheres. Let, therefore, A represent the attraction of a particle of matter, and R the repulsion of its calorific atmosphere; and according as A is greater or less than R, we shall have A − R for the force with which it attracts or repels another like particle. But in the gaseous state, the repulsion, according to Newton, is $\frac{B}{x}$; in which x is the distance and B a coefficient uniformly the same for the same temperature. Therefore $R - A = Bx^{-}$. In this expression we are to observe that B is not a function of x, but of some other independent quantity: for instance, of the temperature of the air. Consequently, if we imagine R to diminish until it be less than A, this diminution cannot alter the form of the function, and it will therefore thence be $R - A = -Bx^{-1}$. That is, the repulsive force varying inversely as the distance will be changed into an attractive following the same law; supposing, as it commonly does in algebraic functions, that Leibnitz's idea of Continuity holds good. Nor could the attraction be reciprocally proportional to the square of the distance, unless the calorific atmosphere was entirely taken away; that is, unless the body was absolutely cold; which we do not admit. We may therefore infer, that if the law of gaseous repulsion be inversely proportional to the distance, no diminution of the calorific atmosphere can change the repulsion into an attraction inversely proportional to the square of the distance; and, conversely, if the attraction be reciprocally proportional to the square of the distance, no increase of caloric can produce a repulsion varying as the distance inversely.

By the same method of reasoning it may be shown, that no law of repulsion differing from the reciprocal duplicate of the distance, can by a diminution of caloric, produce an attraction following this law, that is the law of gravitation. But if the particles repel each other by forces inversely proportional to the square of the distance, the cubes of the elasticity will be as the biquadrates of the density; that is, the elasticity will increase faster than the density; which does not accord with experiment.

This will be the case on the supposition that the repulsion of the particles extends and confines itself to those particles that are nearest them: a supposition which by no means adds to the probability of the hypothesis. Should the repulsion be diffused to an indefinite distance every way about: or should it reach to a certain extent, so that a greater number of forces act upon a given particle in a greater density than in a less, the elasticity will increase or diminish in a still greater ratio than the density. Experience however tells us that, the temperature remaining the same, the elasticity of any air is directly proportional to its density. Therefore the hypothesis of a repulsion in the inverse duplicate ratio of the distance does not agree with phænomena.

The only chance of probability, which it appears to me is left for the hypothesis of caloric, is to give to the calorific particles a repulsion reciprocally proportional to the square of the distance. Any body placed within a spherical homogeneous atmosphere of this kind, would be repelled by a force varying as the distance inversely from the centre; and placed without that sphere, it would be repelled by a force reciprocally proportional to the square of the distance. Two particles with this hypothesis may be so placed that they shall mutually repel with forces reciprocally proportional to their distance; and, if the atmosphere be diminished, shall at length have their repulsion converted into an attraction reciprocally proportional to the square of the distance. But even here conditions are necessary which make it impossible such an hypothesis can be correct. For instance, there must be an attractive atmosphere always of the same dimensions as the repulsive, otherwise the law of repulsion would not at all places within the atmosphere be inversely as the distance. The air also, if ever condensed more than eight times, will have its density and elasticity follow different laws; and if rarefied more than eight times, the cubes of the elasticity will be as the biquadrates of the density. This latter law will likewise take place if the temperature or calorific atmosphere be so diminished as to place the particles, without changing their distance, beyond that atmosphere.

I know there are those who to meet the difficulties of caloric will stir up their "latent heat." But I have shown in the Annals for December, that all the phænomena for which this hypothesis and that of capacity were created, can be truly and mathematically explained without the assistance of either. To press, therefore, into our service, for the explanation of one thing, an hypothesis uselessly created for another, is a piece of tyrannical obtrusion. Allowing however to philosophers the entire powers of "latent caloric;" and granting them the wide field of unrestrained imagination, I think I may venture to say, there is not in Europe a philosopher, who could successfully explain, with this doctrine, the simple developments of untrammelled experiment.

Viewed in this light, it is impossible that repulsion can exist with caloric. And if we try to meet experiments by any alteration in the homogeneity or extent of the atmospheres, we destroy the law which nature observes, and plunge the hypothesis into still greater difficulties. Under no circumstances, therefore, will repulsion agree with caloric and phænomena united.

Repulsion is equally as unfortunate with the theory of heat by motion, as with the doctrine of caloric. If in airs we conceive a vibratory motion of the particles to be the cause of heat, no increase or diminution of this motion could at all affect the elasticity of the air, as experience proves an increase or diminution of heat does, unless the particles struck one another. For, whatever may be imagined to be gained in action or elasticity by an increase of celerity of any particles towards each other, or by a further approximation occasioned by a greater range of vibration, must be counterbalanced by the same increase of velocity from each other, or by an equivalent excess of elongation: so that the mean action or elasticity must be the same under one temperature as under another. And if instead of a vibratory we have recourse to a rotatory motion of the particles about their own axes, we have no physical principles, nay, not even a fact to my knowledge, that will enable us to entertain merely the possibility, to say nothing of the probability, of a change in such motion being able to affect the repulsion of distant unconnected bodies. We may as well affirm that the rotation of two tops at a distance from one another would make them recede; a circumstance so palpably absurd, that we might almost say every child who can spin a top knows it to be wrong.

As to any other motions; for instance, revolutionary motions of the particles about each other, they cannot exist with repulsive forces; because, to maintain periodical revolutions, anything like the planetary, evidently requires centripetal not centrifugal forces.

Whatever view therefore we take of the subject, whether we assume heat to be a substance as caloric, or to be motion, repulsion does not explain even a small part of the properties of aëriform bodies. We may consequently conclude, that repelling forces have not been by nature selected for the production of aëriform phænomena.

The following piece was sent to the Editor of the Annals, to be annexed to my "Remarks," &c. published in the Annals of Philosophy for May; but it seems it arrived too late to be added to that paper; namely, after the sheets were made up. Characteristical explanations, besides what are contained in the piece, are to be found in the original paper, Annals for May.

Let G be the specific gravity of the gas and vapours over water, V the volume, and P the pressure or elasticity of the mixture. Then

$V. \left\{ G - \tau'. \frac{f+448}{48.(F'+448)} \right\}$ = the weight of gas in the mixture; and $V.\frac{P-\tau'}{P}$ = the volume the gas would have under the pressure P. Therefore the specific gravity of the dry gas $= \frac{P}{P-\tau'}. \left\{ G - \tau'. \frac{f+448}{48.(F'+448)} \right\} = g$. Whence

$$G = g + \tau'. \left\{ \frac{f+448}{(F'+448).48} - \frac{g}{P} \right\} = g + \tau' \left(\frac{G'}{48} - \frac{g}{P} \right),$$

by putting G' for the specific gravity of the atmosphere at the temperature F'.

From this theorem we gather, that, when g is less, equal to, or greater than $\frac{P}{48}G'$, the specific gravity of the mixture is greater, equal to, or less than that of the dry gas; and that when g is about $\frac{5}{8}$ of G', the error that would arise from neglecting the influence of the vapour, is very small in estimating the specific gravities; and on the contrary, much greater in gases considerably lighter or considerably heavier. So that hence such an error in ammonia would fall much short of that in phosgene gas, and still more short of that in hydrogen.

Again, in gases considerably lighter or heavier than ammonia, as hydrogen or hydriodic acid, the lower the temperature of operation generally the less would be the influence of vapour; whilst in gases with about the specific gravity of ·6 it may happen that diminishing the temperature would augment the error.

J. Herapath.

IV. *On the Solar Eclipse which took place on September 7, 1820.*

By F. Baily, *Esq. F.R.S. and L.S.**

The solar eclipse of the 7th of September last having excited general attention throughout Europe, on account of its magnitude, I shall venture to lay before the Society such observations as I myself made relative thereto, and also the result of such observations as have been communicated to me by others, whose accuracy I have no reason to doubt. These latter however are at present neither so numerous nor important as I had reason to expect, considering the number of good observers, who must have witnessed this phænomenon: nevertheless I flatter myself that the observations of such persons will eventually be communicated to the public in some other manner.

My own observations were made at Kentish Town, near the bottom of Highgate Hill, in N. lat. 51° 33′ 34″, and W. long. 35″,2 in time, from Greenwich. The state of the clock was determined by several altitudes of the sun, taken on the morning and evening of the 6th, 7th, and 8th, with a Troughton's reflecting circle; the results of which agreed with each other to great exactness. The following are the times of the beginning and end of the eclipse.

	h	′	″	
Beginning =	0	21	42,4	mean time at the place.
End =	3	13	41,1	
Duration =	2	51	58,7	

In noting the time of the beginning of the eclipse, I have not made any allowance for the first second or two of time, which must (I think) in all cases elapse before the commencement of the eclipse can become visible to a spectator, even with the best telescopes. With respect to the termination of the eclipse, I do not consider any such allowance to be necessary, as the eye can follow the moon till it is completely off the sun's disc. The telescope, made use of, was a 3½ feet refracting telescope by Tulley, with an object-glass of 3¾ inches diameter, and magnifying 38 times: but the object end was covered with a brass cap, which reduced the aperture to two inches. The eye was protected from the rays of the sun by a dark glass of a *red* colour; a circumstance which I have thought proper to mention in this place, as it appears, from the remarks of M. Messier, *that the colour of the glass is not immaterial in observations of this kind.*

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* From Memoirs of the Astronomical Society of London.

The sun was perfectly free from spots during the whole of the day; and had been so for the day or two previous thereto. Soon after the commencement of the eclipse, a succession of flying clouds prevented any correct measures being taken of the enlightened part of the sun's disc. Towards the middle of the eclipse however the clouds dispersed; and I had an excellent opportunity of measuring the diameter of the moon on the sun's disc,* with one of Troughton's spider-line micrometers, attached to the telescope. By placing the two lines of the micrometer as tangents to the moon's disc, I found that the distance between them was 41·20 revolutions. But this was evidently too great by the thickness of one of the lines, which I found to be equal to five divisions: therefore the diameter of the moon was only 41·15 revolutions. The value of each revolution (by taking as a standard, the diameter of the sun on that day, as given by Delambre's tables,) was 42″,999: therefore the apparent diameter of the moon, in the direction in which it was measured, was 29′ 29″,4. But this direction was inclined to the horizon about 75 degrees: which (on account of the refraction) diminished the true diameter in that direction exactly 1″,0: so that its apparent diameter, measured horizontally, would be 29′ 30″,4; and consequently its semidiameter equal to 14′ 45″,2.

Now the horizontal semidiameter of the moon, at noon on that day was, according to Burckhardt's tables, 14′ 41″,02; to which must be added 8″,71 for the augmentation at 2 o'clock (the hour of observation): thus making the apparent semidiameter at that time equal to 14′ 49″,73, or 4″,53 more than the above observation. I would here remark that, according to Burgh's tables, the semidiameter of the moon was 14′ 43″,13: which, allowing for the augmentation, would make the apparent semidiameter 6″,64 more than the above observation.

After the middle of the eclipse (the atmosphere remaining beautifully clear) I proceeded to measure the enlightened part of the sun's disc, or the distance of the borders of the sun and moon, with a telescope fitted up for the occasion by the Rev. Dr. Pearson, with a Rochon's prismatic micrometer. It was only 19 inches long, 1 inch diameter, and magnified about 30 times: but it was admirably adapted for the purpose intended. The prism moved through the whole length of the tube by means of a rack and pinion; and took in a scale of

36′, divided into seconds by means of a vernier. The advantages attending an instrument of this kind, are its convenient size, and the ease and expedition with which the observations can be made. Nevertheless I think it right to remark that, in the present instance, it is probable there is a constant error of a few seconds affecting the results, arising from the indistinctness of the borders of the sun and moon, which prevented me from determining the exact point of contact. But as the junction was always made under the same apparent circumstances, the proportion between the results will not be affected thereby. This inconvenience may probably be overcome in any new one made on this construction. The following are the observations which were made with this instrument.

Mean time at the place.	Distance of the borders.	Mean time at the place.	Distance of the borders.	Mean time at the place.	Distance of the borders.
h ′ ″	′ ″	h ′ ″	′ ″	h ′ ″	′ ″
2 17 18	11 8	2 33 14	17 4	2 45 11	21 22
21 3	12 27	34 44	17 30	48 36	22 37
24 13	13 41	36 34	18 11	50 58	23 30
26 10	14 25	39 7	19 4	52 47	24 10
28 2	15 5	40 47	19 43	54 0	24 38
31 22	16 20	43 20	20 40		

At 2h 54′ I left this instrument in order to prepare for observing the termination of the eclipse as above stated: and I cannot but consider it as extremely fortunate that the sun was entirely free from clouds both at the beginning and end of the eclipse.

All these observations were made from a large window; near which was suspended a barometer, with a thermometer attached: and I had also suspended another thermometer in the open air, in the shade. At noon these instruments stood as follow:

Barometer ... ... = 29·86
Thermometer, within ... = 65°
———— without ... = 67°

During the progress of the eclipse, I watched the state of them, but *could not observe any alteration* in either of them. As soon as the eclipse was ended, I again noticed them more particularly, and they stood as follow:

Barometer ... ... = 29·87
Thermometer, within ... = 66°
———— without ... = 68°

The diminution of light was very trifling, and would scarcely have been perceptible, had not my attention been called to it.

It

It by no means appeared so great as the diminution which took place in November 1816: although in that eclipse only ·78 of the sun's disc was obscured, whereas in the present one ·87 was obscured. But the former eclipse I observed through the dark atmosphere of London, where the abstraction of a small portion of light is easily perceptible. And I understand that, in the present eclipse, the diminution of light during the middle of the eclipse was very perceptible in the metropolis, and at places where the sun was obscured by clouds. Venus was seen by thousands of spectators with the naked eye: and I am informed that Mars also was visible to many.

Mr. Dollond informed me that he took the horizontal diameters of the sun and moon, at Greenwich, with one of his divided object-glass micrometers; and that they were in the proportion of 3·351 to 3·103. Therefore, assuming the semidiameter of the sun, as deduced frem Delambre's tables, as a standard, the apparent semidiameter of the moon will be 14′ 44″,14; being about one second less than my own observation. The times at which Mr. Dollond and Mr. Taylor observed the commencement and end of the eclipse, at the Royal Observatory, were as follow:

	Dollond.	Taylor.	
Beginning	= 0^h 22′ 37″	0^h 22′ 33″,6	mean time at Greenwich.
End	= 3 14 40	3 14 44 ,5	
Duration	= 2 52 3	2 52 10 ,9	

Mr. Groombridge has favoured me with the following observations of the eclipse at Blackheath: N. lat. 51° 28′ 2″, E. long. 0″,67 in time from Greenwich. End of the eclipse at 3^h 14′ 34″ mean time at the place: the beginning not accurately observed.

Vertical distance of the Cusps.

Mean time at the place.	Rev. of Microm.	Mean time at the place.	Rev. of Microm.	Mean time at the place.	Rev. of Microm.
h ′ ″		h ′ ″		h ′ ″	
0 42 12	12·61	2 17 16	35·94	2 43 22	27·84
1 23 44	20·81	20 30	34·78	45 20	26·89
47 4	8·11	22 38	34·16	48 4	25·84
50 36	15·89	25 32	33·45	53 18	23·57
2 1 40	39·28	29 8	32·38	56 46	21·85
5 6	41·11	31 20	31·64	59 0	20·50
9 33	37·01	34 28	30·47	3 0 36	19·39
13 5	37·05	40 8	28·87	9 41	7·28

Each revolution of the micrometer was equal to 44″,982.

The Rev. Dr. Pearson measured the diameters of the sun and moon not only with one of Dollond's divided object-glass micrometers, but also with one of Troughton's line-micrometers. By means of the former he made the moon's semidiameter equal to 14′ 44″,6; and with the latter, equal to 14′ 44″,7: the semidiameter of the sun being considered as the standard for the scale. These measures correspond with my own, prior to their reduction. Dr. Pearson also measured the luminous portion of the sun, when most obscured, by means of one of his compound prismatic eye-pieces with variable powers, attached to a 2½ feet achromatic telescope, and found it to be 3′ 58″,24. This measure would indicate an error in the lunar tables; as the eclipse ought not to have been of this magnitude even at Greenwich; and much less ought it to be so at the place where the observation was made. The distance between the cusps at 1h 53′ was 28′ 53″,8 by Dollond's micrometer: and the distance of the cusps was exactly equal to the diameter of the moon, on its leaving the sun's disc at 2h 2′ 25″. The end of the eclipse took place at 3h 13′ 20″ mean time at the place: the beginning was not observed. Dr. Pearson's observatory is situated at East Sheen, in N. lat. 51° 27′ 35″,7, W. long. 1′ 3″,7 in time from Greenwich.

Mr. William Allen observed the eclipse at Stoke Newington N. lat. 51° 33′ 40″, W. long. 22″ in time from Greenwich.

Beginning	=	0h	22′	31″
End ...	=	3	13	59
Duration	=	2	51	28

Mr. Isaac Wiseman wrote to me from Norwich (N. lat. 52° 38′, E. long. 5′ 10″ in time from Greenwich), stating that the eclipse began there at 0h 28′ 45″, and ended at 3h 21′ 40″, mean time at the place. The observation was made with a three feet reflecting telescope, with a power of 180; and the time was deduced from a meridian of his own construction. This gentleman has also sent me the result of some experiments on the power of the burning lens on different substances, during the time of the eclipse. Having procured a piece of pasteboard, he affixed thereto four equal pieces of different coloured cloths; viz. black, blue, yellow, and red; and placed them successively in the focus of a burning lens, *on the day preceding the eclipse*. The following are the periods at which they respectively took fire: viz.

Black	in	7″
Blue		7
Red		8
Yellow		16

He also on the same day submitted the bulb of a thermometer (which then stood at 66°) to the focus of the lens; and in 1¼ minute it rose to 94°, and probably would have risen higher, had he not been apprehensive that the glass would have been broken by the heat. These experiments were made at about two o'clock in the afternoon, in order that they might correspond with the time of the eclipse at its greatest obscuration. On the following day, about half an hour after the commencement of the eclipse, he applied the cloths in succession to the focus of the lens, and found the periods, at which they respectively took fire, to be as follow: viz.

Black	in	20″
Blue		20
Red		16
Yellow		40

At about half an hour before the end of the eclipse he again submitted them to the focus of the lens, and found their periods of ignition to be as under: viz.

Black	in	17″
Blue		18
Red		14
Yellow		24

But during the time of the greatest obscuration he could not produce any effect on them whatever. The thermometer at the commencement of the eclipse was at 66°; and by two o'clock had fallen to 61¾. This was about the middle of the eclipse: and Mr. Wiseman assures me that at this time he *held the bulb in the focus of the burning lens for upwards of four minutes, but without producing any sensible effect.* At a quarter past two, he repeated the same experiment, and with the same result, although the sun was free from clouds. At the termination of the eclipse the thermometer rose to 64°. Mr. Wiseman also states, that he fitted up a prism in a darkened room, and that he made several observations on the coloured rays, which were thrown on a screen of white paper. He says that, during the continuance of the eclipse, the yellow and blue rays were generally increased in brilliancy, whilst the red became exceedingly faint, and did not occupy more than half their usual breadth. As I am not aware that any experiments of a similar kind were made during this eclipse, and as the results are somewhat singular, although anticipated by Mr. Wiseman, I have thought it right to state them here in order that the attention of the public may be excited thereto in any future eclipse.

Mr. Sloane of Belfast informs me that the eclipse commenced there at 11^h 47′ 38″ mean solar time: the observation was made with one of Dollond's achromatic telescopes, magnifying about 75 times.

At Bury in Lancashire, the eclipse commenced at 0^h 9′ 10″,5 apparent time at that place, as observed with an achromatic telescope of five feet focus. The latitude of the place was 53° 35′ 30″, and its longitude west of Greenwich 9′ 8″ in time.

Most of the letters which I have received from the country remark that the diminution of light was not so great as was expected. The fall of the thermometer towards the middle of the eclipse, was various in various places. I have already stated that, as far as my own observations extended, I could not perceive any diminution: the inspection of the instruments was made at intervals during the eclipse. In some places, I am informed, the fall was as much as 10°; and, where the thermometer was placed in the sun, as much as 15°. It appears that the power of a lens to ignite gunpowder, was suspended from 10 to 15 minutes, during the middle of the eclipse: and it has been already stated, that for about the same period the lens was incapable of producing any effect on the thermometer:—an experiment which I believe is new, and which is certainly worthy of repetition, whenever another eclipse of any considerable magnitude may present itself.

From the continent I have received some communications, which tend to confirm the observations made by former astronomers on this singular and rare phænomenon.

At Frankfort on the Maine, Mr. J. V. Albert observed the eclipse, as follows:

Beginning	1^h 14″,0	Apparent time.
Do. of the annulus	2 37 ,0	
Middle of do.	2 39 ,45	
End of do.	2 42 ,30	

At the observatory of the Grand Duke of Baden at Manheim, M. Nicolai observed the eclipse as follows:

Beginning of the annulus	2^h 37′ 37″,8	Apparent time.
End of do.	2 42 32 ,0	
End of the eclipse ...	4 0 50 ,0	

The actual formation of the annulus was very remarkable: for, about a second before it occurred, the fine curve of the moon's disc, then immediately in contact with the edge of the sun, appeared broken into several parts: and in a moment these parts flowed together like drops of water or quicksilver placed near each other. At the dissolution of the annulus, a similar appearance presented itself: for the delicate thread of light then formed by the annulus, instead of being broken in *one* place only, was in an instant divided in *several* places at once. The thermometer (reduced to Fahrenheit's scale) was at the commencement of the eclipse at 66½, and fell towards the middle to 63, but afterwards rose again to 66½.

At Augsburg, Professor Stark observed that the duration of the annulus was 5′ 47″,5; but neither the beginning nor end of the eclipse could be observed on account of the unfavourable state of the atmosphere: Reaumur's thermometer fell 3½ degrees (equal to 8° of Fahrenheit).

At Spire, Professor Schwerd made the following observations:

Beginning of the annulus	2ʰ 37′ 55″,5	Apparent time.
End of do.	2 42 43,5	
End of the eclipse ...	4 0 57,1	

About six seconds before the formation of the annulus, a bright spot was seen on the point of one of the horns, which shortly after appeared to flow into it. About half a second before the complete junction of the two horns, there appeared a row of bright points. A similar appearance was observed at the dissolution of the annulus. The barometer stood at 28,1 inches: and the thermometer (reduced to Fahrenheit's scale) fell from 69¾ to 64. A burning-glass, six inches diameter, which immediately set wood in a flame, did not ignite tinder during the time of the middle of the eclipse; nor would it turn paper, in the least, brown.

At Munich (in the middle of the city) the formation of the annulus was observed at 2ʰ 53′ 23″ mean time. The barometer stood at 26,78 inches. And the thermometer (reduced to Fahrenheit's scale) fell about three degrees.

From the island of Zante I have received communications from two observers; differing in some trifling points from each other. But as the results are stated to the nearest *minute* only (omitting the seconds), I do not think it necessary to quote them in this place. It appears however that the atmosphere was not perceptibly darkened, till nearly the time of the formation of the annulus, which lasted about five minutes: and that the thermometer fell only 2½ degrees during the whole time of the eclipse; viz. from 88½ to 86.

V. *Account of a successful Experiment to prevent the injurious Effects resulting from the Diffusion of arsenical Vapours from Copper-smelting Furnaces**.

THE injurious effects of the escape of arsenical and other deleterious vapours from the copper-smelting furnaces in the neighbourhood of Swansea have been long known. These condensing and falling on the surrounding country, have not only greatly injured, but almost totally destroyed, vegetable and animal life in that vicinity. Thousands of acres are rendered useless entirely from the arsenic falling in showers upon the surface of the land; and, indeed, numberless instances have occurred of the teeth dropping from the mouths of cattle that have grazed in the neighbourhood of these furnaces, and, upon examination of such teeth, they have been found to be coated with a strong crust of copper. Several chemical and mechanical projects have been adopted to prevent the escape of these substances from the chimney of the smelting furnace; and although their effects have been confined within the works, yet it has been found attended with considerable trouble and expense, which the copper-smelters seem unwilling to adopt in their daily practical operations. "The Cadoxton experiments," as stated in this report, seem fully to remove the objections of the smelters, and yet obviate all the inconvenience to the surrounding country. The discovery was first announced by the following paragraph in the Swansea paper:

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* Monthly Magazine for May 1822.

"An experiment has been made at Cadoxton, in Glamorganshire, for obviating the inconveniences arising from the calcining and smelting of copper ores, by destroying the noxious qualities of the smoke from the furnaces upon the whole process, and by destroying or reducing as much as possible the bituminous smoke, upon a plan adapted to the present practical operations of copper-making, and without increased expense to the manufacturer."—*Cambrian, Nov.* 24, 1821.

The principle aimed at in this experiment is simple *precipitation;* the mode of effecting it by gaining as much *time* as possible between the production of the smoke in the furnace and its final exit into the atmosphere; in short, by imitating as nearly as possible the condensation of vapour in a still, where the worm (presenting the greatest possible surface in the smallest possible space) may be considered the flue: if the flue in this case could be conveniently passed through a cold medium (as the worm of a still), the imitation would be still nearer; but it should seem, from the result of this experiment, that it is not necessary. The experiment was as follows:

A calcining furnace (which throws out the greatest portion of noxious ingredients) of the ordinary dimensions has been erected; instead of a short perpendicular flue, an horizontal flue was carried from it on the surface of the ground, or rather a set of connected flues, 24 in number, consisting of straight parallel lines rounded off at the ends, and furnished with doors for the purpose of observation, each line being 34 feet in length; from the end of this line the flue was continued for some distance to the pits of a neighbouring lime-kiln 22 feet deep, which was furnished with a brick cap in the shape of a cone, terminating at the top like a common chimney, and making an upright vent of 50 feet, the whole length of the flue being 950 feet; the bottom of the flue rises about an inch in every 34 feet, upon the principle that a regular ascent was necessary to indulge the propensity of the hydrogen to ascend, and thus facilitate its passage forward, whilst it was at the same time desirable to keep the smoke confined to a passage as nearly horizontal as was consistent with that principle; for, in a former experiment, it had been found that, from the want of this caution, and in consequence of some descent in the flue, the hydrogen accumulated in it so as to burn like gas on the application of a light, and on one occasion to cause a violent explosion.

In the first experiment with the new flues a *ton of copper* was placed in the furnace, when at a proper heat, and the process of calcining commenced: the following account is extracted from the letter of the gentleman who reports upon it:

"The smoke issuing from the vent was perceptible only to a good eye looking against a wood behind it; two persons ascended to the top of the perpendicular stack, whilst a workman stirred the ore in the furnace: whilst this was doing, two observers sat on the edge of the stack on the lee side, so that all the smoke which issued must pass over their faces; and they state that they found no kind of inconveniences when seated on the mouth of the chimney." The reporter says, "Whilst I stood by the furnace I could perceive a small issue of smoke, which appeared to hover for an instant at the mouth of the stack, but was dissipated very soon after it had entered the atmosphere. I went up to the stack and into it by the door, there I found a mixture of smoke and hydrogen gas, smelling like common smoke; but I inhaled it repeatedly without experiencing any of those distressing sensations which always affect me when I inhale copper smoke, and to which you were once witness, as we walked by the crown works on a day when, the smoke being remarkably low, it was impossible to avoid it. I immediately went into the garden and procured a common plant in a pot in full vegetation; this I had placed on the summit of the stack, and the superintendant tells me this evening, that it does not yet appear at all affected; indeed, he declares it impossible that it should be; he says he has no doubt remaining on the subject, '*the thing is accomplished*, and nearly all the smoke which does escape is combustible;' he adds, 'it is evident that 950 feet of such flues are sufficient to destroy all the noxious properties of the copper works.'"

This experiment was made on the 23d of March 1822. On the 25th, the reporter proceeds as follows:—"On opening the flues this morning, the first and second were found charged with soot and a white crust, probably arsenic, and sulphur over the bottom and lower part of the sides. Nos. 3, 4, and 5, presented soot, with a superstratum of sulphur, in considerable quantities. Mr. Young thinks that the three flues contained from ½ cwt. to 1 cwt. of deposit, the 6th and 7th soot, and a smaller quantity of sulphur; the 8th coal-tar, which continued to form on to the 14th, gradually diminishing, accompanied by very little sulphur. From the 16th to the end, there was no deposit worth notice, and the bricks and mortar of the 24th were not even discoloured. On lighting the furnace to-day the smoke was ten minutes in reaching the mouth of the stack. After the fire was well up, we threw in a bundle of wet straw, and observed that the smoke arising from its combustion appeared at the mouth of the stack in three minutes." The reporter proceeds to observe, "It now appears, in the undeniable shape of experimental fact, that the noxious parts of copper-smoke may be effectually controlled and compelled to stay *in-doors* without any chemistry besides that which nature furnishes. Bricks and mortar, and a tolerable mason, are all the array of power and science which need be called into action. The draft in your flues is so perfect, that Mr. ——, a manager of copper-works, who furnished the ore, cautioned the man against allowing the fire to become *too intense for the process of calcination;* and there is no doubt that several hundred feet of flue might be added without impeding the draft. Mr. ——, however, thinks that all which now escapes may be consumed by combustion: we have no doubt on the subject."

The writer of the foregoing letters, having been warned against trusting too implicitly to first impressions, on a single attempt to reduce principle into practice, writes on the 28th March as follows:—"In the detail which I sent you, I believe I did not observe, as I should have done, that the ore was as well calcined in as short a time, and without more fuel, as is used in the ordinary flues;—this by way of supplying an apprehended omission. Now for the objections which have presented themselves, either in the shape of my own ideas or the *sayings* of others. It did occur to me that the *newness* of the flues might operate favourably in expediting the process of condensation and precipitation. It was also said, that the fresh mortar would have a chemical action on the acid particles of the smoke, and thus neutralize one part of the mischief. A question has also been started, whether there can be sufficient draught obtained through such a length of flue to *melt* the ore after calcination. On this last point I have felt doubts, but the superintendant says he feels no doubt on the subject; and states, that, if any such difficulty should arise, he would shorten

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the passage without apprehension, because in the process of melting there is far less offensive matter disengaged, and of course *less to precipitate, than in calcination.* It is fair also to take into consideration that our stack or chimney is not the most favourable in its formation, being very open and much wider than the flues. He argues further, that the construction of a smelting-furnace is calculated to increase a draught to the utmost, whilst that of a calciner is calculated to check its direct velocity, because an intense heat would be improper for that part of the process. Now the calciner at Cadoxton became [illegible], when the fire for drying the flues was at its height, that the [illegible] *red* hot,—a degree of heat far too great for calcination, and this too, let it be observed, *with your own coal**, which would do nothing with the old flues, and which the copper works could not use without a mixture of bituminous coal.

As to the objection on account of the *newness* of the flues, I cannot refuse my assent to the argument with which it is rebutted by the superintendant: he says the flues had smoke from a *roaring* furnace passing through them for 48 hours before they were used in the experiment, being composed of bricks and thin tile-stone; the former only on edge, and with no more mortar between them than serves to close the interstices; such a process would be likely to exhaust a large portion of their moisture. Then, as for the *new lime*, that also was become nearly dry, and was in too small a quantity to produce any specific effect. Then we come to the facts developed on inspecting the flues: they were coated with the matters deposited, consequently the *surface* of this coating could not be affected by the substances beneath the *substratum* of deposit: however, those and all other objections ought to be fairly met, and this can be done only by a perseverance in the experiment. We have spoken to several intelligent gentlemen, who will come and give us their remarks on the next calcination.

From this report there appears good reason for believing that the principles, exemplified by this experiment, will put an end to a nuisance more destructive to animal and vegetable life than any other existing in this kingdom.

The deposit from the regular copper-works in the neighbourhood of Swansea and Neath (in which many chaldrons of coals are consumed every hour, day and night, throughout the year) must be immense, and it will be matter of curious inquiry, whether this deposit may not be turned to profit by the manufacturers. It has been found to yield by analysing three per cent. of fine copper, and the arsenic, sulphur, and other substances which abound in copper ore may possibly be made profitable; even the spare heat on so large a surface as these horizontal flues cover, may present a *climate* for trades requiring a moderate degree of heat, or for hot-houses, glass frames, &c. Time will probably develop great improvement in this branch of trade, which has remained stationary perhaps for a century past.

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* The coal here alluded to is a sort of inferior culm, fit for little else than lime-burning: it has little or no binding quality, and will not coke; so that without a mixture of binding coal it has always been considered unfit for the copper-smelter.

This short description, however imperfect or unscientific, is submitted, on the impression raised by the reports referred to, for the purpose of inducing intelligent and scientific men to turn their minds to this very interesting subject. The mischiefs produced by the present system of smelting copper can scarcely be conceived by a stranger unacquainted with its destructive consequences.

Note by the EDITOR.—When a cure is propounded for a disease, the magnitude of the evil is generally somewhat exaggerated by those who put forth the remedy. So it is, we are told, with the smoke of the Copper-Works at Swansea, which though undoubtedly a nuisance to a certain extent, are said not to produce such dreadful effects as are set forth in the commencement of this paper. It is certainly injurious to vegetation; but, as we have lately been informed, the traces of this are confined to a narrower limit than might have been imagined by any one acquainted with the great volume produced. With regard to health and animal life, there is no evidence, that we ever heard of, that would lead us to think that injury results from the furnaces of the common construction; and according to the statement of a medical man near the works, it appears that disease is not frequent, and that instances of longevity are common, a fact which is confirmed by experience in the vicinity of copper-works in another county.

The assertion that the teeth of animals grazing near the furnaces have been found to be coated with copper, we must beg leave to doubt, not believing that this metal is volatilized at all in any sensible degree; or if it were, that it would be found just in such a situation.

The Cadoxton experiment, described in the foregoing paper, is no more than the application to copper calcining furnaces of what is very common to those of tin and lead; for proof of which we refer, first to our Number for last month, where in Mr. Taylor's paper on the Smelting Tin Ores in Cornwall, p. 420, Vol. 59, it is said that the arsenic "*is condensed by long horizontal*

horizontal flues constructed for this purpose." Secondly, we would mention as a matter of notoriety, that most of the lead smelting-furnaces in different parts of the kingdom are furnished with such flues, to condense, as far as it can be done, the lead which is evaporated in the process of smelting.

Such flues as are described in the foregoing paper will, doubtless, condense the arsenic, and are therefore used in the Tin-burning houses in Cornwall. But we must observe that this substance is not the greatest difficulty to overcome in obviating the evil. The destructive effects, such as they are, are chiefly to be attributed to the sulphur, which is much more abundant in copper ores than arsenic, and is driven off in a gaseous form, and therefore very difficult to condense, and totally out of the reach of such an apparatus as is now described; unless, indeed, this condensation might be effected, as is suggested by the lime exposed in a new building, and which would terminate as soon as the mortar was saturated.

The account is deficient in one respect, it does not allude to a fact which should not have been suppressed. It is, that with a view to accomplish this very object of abating the nuisance arising from the Copper-works near Swansea, a reward of 1000*l.* was most liberally offered by the gentlemen having property in that neighbourhood; and that this and other circumstances led to the consideration of this subject some time since; and that therefore public attention was drawn to it, and the ingenuity of many was directed towards this object before the Cadoxton experiment was heard of.

No mention is made of the experiments making by Messrs. Vivian and Sons at their great smelting-works, although they are upon a scale of magnificence, and constructed with such a scientific consideration of all circumstances, as may well be expected to ensure success as far as it is attainable. There is a liberality, too, in Mr. Vivian's proceeding, which cannot be too highly praised in a public affair of this kind; for he not only has declared his intention of not being a claimant for the reward, even if his plan should prove to be the best, but is actually a large subscriber to the sum that is intended to stimulate the exertions of others.

VI. *On the Connexion between the Leaves and Fruit of Vegetables; with other physiological Observations.* By ANTHONY CARLISLE, *Esq. F.R.S.*[*]

IN the art of gardening, it must be allowed that the greatest value will always belong to special facts, noted down by experienced, acute, and judicious men. Still, however, the mind, which is constantly occupied upon particular details, may not be equally awake to those affinities and connexions in nature, from whence useful general rules may be deduced; for it is by a comprehensive classification of leading facts, that abridgements of knowledge are made, and these must form the boundary to unprofitably minute particulars. This gathering together of similar evidence, and the assortment of it into systematic order, are more within the province of the inspector than that of the experimenter. Under those impressions, the following observations are submitted to the Horticultural Society, trusting that scientific or practical men may employ the accruing suggestions so as to enlarge the scope of this branch of useful knowledge. Without presuming on the general series of facts which I have to mention, being free from apparent exceptions, I offer them as an extensive range of coincidences, from whence beneficial inferences may be drawn. There is an obvious connexion between the kind of foliage and the kind of fruit in many of our orchard and garden trees; and although the full and true uses of the leaves of vegetables are not well understood, yet the subject seems to be open to research, and even now capable of practical applications. The first remarkable coincidence is, between the size of the leaves and the size of the fruit. This may be seen in the contrast between the fruit and the leaves of the *Magnum-bonum* plum, and those of the *Damascene*, the *Bigarreau*, and the wild cherry; the *Cadillac* and the little musk-pear; the yellow Antwerp and the red prickly raspberry; the several varieties of the gooseberry; the common small walnut and the large or double walnut; the large and small medlar; the different species of cranberries; the filberd and the hazel nut. And the same indications prevail in herbaceous fruit-bearing plants; as in the melon and gourd, the strawberry; and among esculent vegetables, as in the pea and bean.

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[*] From the Transactions of the London Horticultural Society.

By an accurate observance of these characteristics, new seedling varieties may be partly estimated in an early stage, and the known kinds of fruit trees better distinguished when they are not in bearing. Other evident properties in the foliage of vegetables, such as their tints of colour, their hardness, thickness, and capability of resisting cold, are associated with the qualities of their fruits. The local influences of mountainous situation and of a bleak aspect, which stunt the foliage, affect both the fruits and the growth of timber in all trees. The ravages of herbivorous insects, and of parasitical fungi, are thus also indirectly mischievous to fruits. It is probable, that the great differences in fruits, of the same variety, which arise from local causes, are principally dependent on the foliage. Even the diversity of the seasons, between one year and another, gives rise to noticeable alterations in the qualities of the produce from the same trees.

Perhaps a further advancement in the knowledge of vegetable nature may enable the scientific gardener to govern the foliage of his trees by artificial methods, so as to adapt them better to the vicissitudes of locality, and to the production of the more desirable fruits *. It is likely that the unfavourable change induced upon the Downton pippin by a milder locality than that of its native place, may depend upon a more luxuriant production of leaves; and, if so, under such new situations, the good qualities of that fruit might be again restored by reducing its exuberant foliage. The gradual deteriorations arising from old age in trees, are equally evinced upon the foliage and the fruit. It belongs to the peculiar habits of some varieties of trees and plants, to produce a greater or less proportion of leaves than ordinary; and, perhaps, the particularities in their physical properties make some of such leaves more efficient than others; since the differences in their firmness, thickness, and colour, may be reasonably supposed to influence one or more of their vital offices. A remarkable instance of the adaptation of foliage occurs in Winter Spinage, the leaves being of a more dense texture, of a deeper colour, and more compacted about the stems and roots, than in the summer variety †. These and similar evidences may probably indicate the hardiness of other esculent or ornamental plants, and may lead to the introduction of many new, useful, or ornamental vegetables, where they are at present unknown. In the spring of 1815, the first tender leaves, and the immature male catkins of the generality of our walnut-trees, were destroyed by the frosts; the subsequent crop was very few in number, and of indifferent quality. I consider those consequences to have arisen from the destruction of the first shoot of leaves: but there was also in the same season an unusual number of abortive or empty nuts, and this was apprehended to be wholly owing to the want of masculine impregnation, as

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* I am informed by a practical gardener, that buds may be artificially produced upon the long naked spaces of boughs, by making scars into the bark, similar to those left by imperfect leaves, or gem-like stipulæ. Indeed, nature points out the efficacy of such methods, by those clusters of twigs so often displayed in the branches of the elm, birch, and wild cherry, and which result from the punctures of insects.

† The attenuated and delicate foliage of forced hardy plants and housed shrubs, is known to be more readily injured by frost than if they had remained exposed; and the physical differences in the texture of such leaves sufficiently explain the cause of that acquired tenderness.

I have produced the same effect upon filberd- or hazel-nut bushes, by taking away the male catkins before the stigmata were evolved. The well-known fact of clusters of empty nuts, often depends upon the destruction of their neighbouring catkins, and I have seen it extensively produced by a dormouse. A series of experiments, which I began some years ago, upon the different freezing capacities of viscid fluids, induce me to conclude, that many, if not all, of the vegetable powers of resisting congelation, depend on the same or similar physical causes. I was led to this inquiry by noticing the difficulty of freezing treacle, paste, dissolved starch, and some oils, as those from the nut and almond, and the most part of essential oils. The remarkably low temperature required to freeze these viscid compounds, directed my attention to the composition of the juices, and to the parenchymatous parts of the willow, the holly, the misletoe, and the alder; and also to the physical composition of many evergreens. Some of those, as the box, extend over a wide range of climates, and others which have been transported hither from mild regions, bear the frosts of this country with impunity. The causes of this capability in plants of enduring such vicissitudes, is worthy of research, because the results may, as before intimated, teach us how to ascertain those vivacious properties, both in exotics, and in the new varieties of our own culture*.

The advancement of vegetable physiology has been too long retarded by a continued effort to force upon it analogies with the structure and offices of animal organs. The ways of nature are, however, not limited by confined rules like those of man: her diversities are endless; and she is not obliged to work by any methods of system, of analogy, or of classification. Such methods are indeed great (if not indispensable) helps to human knowledge; but when we attempt to impose them upon the works of creation, we often darken and dull the evidences of our senses, and confuse the operations of reason.

The leaves [illegible] have been compared to the lungs of animals, upon [illegible] slender analogies: but the additional offices known [illegible] performed by leaves, show them to be also allied to the digestive and assimilating organs. Nor is it at all unreasonable to inquire whether the digesting, secreting, excreting, and pneumatic operations of life, are or are not compatible with each other in the same part, and whether in some examples one of those functions may not singly belong to leaves, and in other cases many of them be associated together*. The greatest number of resemblances between plants and animals are to be found in those of the most simple structure in both kingdoms; and in all such instances the governing influence of physical causes is strikingly obvious; whereas, under a complexity of organic textures, we are apt to put aside the only natural causes which we are permitted to comprehend, and to attribute the phænomena to an occult cause, known under the ill-defined term, vitality. Assuredly, the leaves of vegetables very generally perform the offices of animal stomachs: as when they convert the raw material of vegetable nutriment into a new and peculiar substance for building up the fabric of plants. The extensive variety of new compounds which different plants contain, are elaborated from nearly the same kind of raw material; and doubtless the vast variety of vegetable leaves is in each species adapted to their special secretions. It is probable, that the novelty of these statements may give rise to controversial opinions; but since I only adduce them as connected with general views, and not as universal truths, my object will be fully attained if they occasion any new series of accurate and decisive experiments, being myself wholly indifferent as to the side on which truth is ultimately to rest, provided it be clearly elicited.

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* The generality of winter buds are admirably adapted for resisting frost, by the compacted and dry state of their spongy substance; the buds of the horse-chesnut are coated with a viscid varnish, which is impenetrable to rain or dew. Most of the other species of winter buds are equally protected against evaporation from within, and moisture from without, by dense shelly coverings.

My individual power, or ability, to prosecute these and similar researches, is forbidden by a hopeless want of leisure. I therefore gladly concede the task to those who are better circumstanced.

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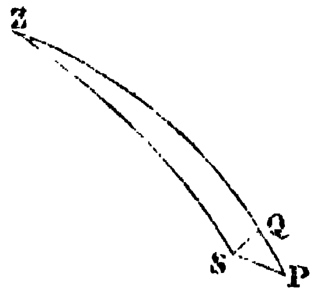
* I wish it to be understood, that I consider the green cellular tissue of the bark in young branches to consist of the same material with their leaves, and that it is devoted to the same purposes: this parenchymatous pulp seems to be a reservoir for the next successions of foliage and fructifications, because buds consist chiefly of fibrous material. The branches of the China-rose possess more of this green pulp than the branches of other roses, and which may be the cause of its continued flowering. May not the stock of the China-rose be therefore more efficient for the healthy support of buds and grafts, from the yellow and other difficult flowering roses?

VII. *Further Remarks on the new Method of determining the Latitude of a Place, by Observations of the Pole-Star.* By FRANCIS BAILY, *Esq. F.R.S.**

IN the last Number of the Philosophical Magazine, I pointed out the method which had been lately suggested by M. Littrow for determining the latitude of a place by observations of the zenith distance of the pole-star, at any part of its diurnal revolution. Since that paper was printed I have re-considered the subject more attentively, and find that that method is not confined to the pole-star, but that it may be extended with equal advantage to *other* stars situated at even a greater distance from the pole, and whose slow motion in altitude renders them peculiarly adapted to such observations.

This will be seen from the following demonstration of the general proposition: and as it does not appear to me that the steps of the process have been very strictly pursued by some of those mathematicians who have treated thereon (since very different results have been deduced by them), I hope I need not apologize for here calling the attention of the reader once more to the subject.



Let Z be the zenith of the place of observation; P the pole; and S the place of the star, in its revolution round P: from S let fall the line SQ perpendicular to ZP. Then we shall have ZS $= z =$ the zenith distance of the star: PS $= p =$ the north polar distance of the star: the angle ZPS $= t =$ the hour angle: ZP $= \psi =$ the co-latitude of the place: and QP $= u =$ the segment formed by the perpendicular. Now z, p, and t being given, we have, by the common solution of the case in spherical trigonometry,

$$\tan u = \cos t . \tan p$$

$$\cos(\psi - u) = \frac{\cos u}{\cos p} \cos z.$$

This solution is general: and it may readily be seen that by the help of seven logarithms, any particular case may be determined. M. Littrow has, with a view to the more familiar and easy solution of the case of the pole-star, thrown the value of p and u into series, by the following well known expressions:

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* Communicated by the Author.

$$\tan p = p + \frac{p^3}{3} + \&c.$$

$$\cos p = 1 - \frac{p^2}{2} + \&c.$$

$$u = \tan u - \frac{\tan^3 u}{3} + \&c.$$

whence, by rejecting all the powers above the third, as of no value (which, in the case of the pole-star, may be safely done) and substituting the series in the general expression above mentioned, we have

$$\tan u = \cos t\left(p + \frac{p^3}{3}\right)$$

$$u = \cos t\left(p + \frac{p^3}{3}\right) - \frac{\cos^3 t.\ p^3}{3}$$

$$= p.\ \cos t + \frac{p^3}{3}(\cos t - \cos^3 t)$$

$$= p.\ \cos t + \frac{p^3}{3}.\ \sin^2 t.\ \cos t.$$

$$\cos u = 1 - \frac{p^2.\ \cos^2 t}{2}$$

$$\frac{\cos u}{\cos p} = 1 + \frac{p^2}{2} - \frac{p^2}{2}.\ \cos^2 t$$

$$= 1 + \frac{p^2}{2}.\ \sin^2 t$$

$$\cos(\psi - u) = \cos z + \frac{p^2}{2}.\ \sin^2 t.\ \cos z$$

$$= \cos z + \frac{p^2}{2}.\ \sin^2 t.\ \cot z.\ \sin z$$

$$(\psi - u) = z - \frac{p^2}{2}.\ \sin^2 t.\ \cot z$$

$$\psi = z - \frac{p^2}{2} \sin^2 t. \cot z + p. \cos t + \frac{p^3}{3} \sin^2 t. \cos t$$

$$= z + (p + \mathrm{C}) \cos t - \mathrm{B} \cot z.$$

which is the very same formula as that which I have given in my former paper.

This formula is an approximate one; but, as far as the pole-star is concerned, is correct: the terms which are omitted not affecting the result in any sensible manner. If the general formula be used, it may be applied with equal advantage to δ *Ursæ Minoris* and other stars not far distant from the pole: and thus the means of deducing the latitude from such observations will be multiplied. But if that formula be converted into series, we must (in these cases) retain the fourth and sometimes the fifth powers of p: which however will not render the approximate formula more intricate or laborious, and the computer may adopt it in lieu of the general one. Yet I doubt the advantage to be obtained thereby; as I shall in the sequel more fully explain.

I have stated that different results appear to have been obtained by different mathematicians in their solution of this problem. Dr. Dirksen makes the value to be

$$\psi = z + p.\cos t - \frac{p^2}{2}\sin^2 t.\cot\psi - \frac{p^3}{2}\sin^2 t.\cos t.\cot^2\psi$$

which differs from the formula above adduced, by having in the third term cot ψ instead of cot z; whilst the fourth term is altogether different. Mr. Horner's formula also, which is much more complex than Dr. Dirksen's, involves the cot ψ. The tables likewise of Mr. Stevens (to which I shall presently allude) as well as those of M. Schumacher, appear to be founded on the same erroneous formulæ. The table also recently published by Capt. Lynn, in page 133 of his "Star Tables for 1823," and which was furnished by an eminent mathematician and astronomer, seems formed on the same principle. The same may likewise be said of the rule given by the Rev. Mr. Lax in Problem XIV. of his "Tables to be used with the Nautical Almanac." It is not easily seen how these different authors can have deduced their formulæ so as to involve expressions depending on the co-latitude of the place of observation; since it by no means enters into any of the steps of the process. It is true that, in the case of the pole-star, and for nautical purposes, this substitution of the *co-latitude* for the *zenith distance* will not be of any consequence. But in the case of stars having greater north-polar distance, and where the observations are carried on in an observatory, the computations must be confined to the correct formula. In fact, since the true values are in all cases *more easily* deduced from the correct formula, there can be no good reason for, at any time, having recourse to an erroneous one. In the example given in my former paper an error of 2″ would arise from the substitution of the approximate co-latitude, for the zenith distance: but in the case of δ *Ursæ Minoris* the error would be 10 times greater from a similar substitution*. These quantities may be of little or no moment at sea, but in an observatory they cannot be neglected.

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* This star, which, after the pole-star, is the next resorted to for observations of the latitude, as well as for the adjustment of the transit instrument, is not one of the stars which are included in the Greenwich catalogue: therefore neither its mean place at the beginning of the year, nor its apparent place at any other time is to be found in the Nautical Almanac. On the Continent, however, the position of this star is considered of so much importance that (like the pole-star) its *apparent* place, both in right ascension and declination, is now calculated for *every day* in the year: an example worthy of imitation in this country.

Since the latitude may at all times be so easily computed from the general formula which I have deduced, it does not readily appear why M. Littrow should have considered it necessary to alter the form of it, by the introduction of the series above mentioned. It is true that we are enabled thereby (as far as regards the pole-star) to compose tables for *nautical* purposes, which may be of great use and advantage at sea; and save much time and labour. But, this does not seem to have been his object: and moreover, in an *observatory*, where the greatest accuracy is required, the approximate formula is always equally laborious with, and sometimes *more* so than the correct one; as may be readily ascertained on trial. For, *six* logarithms are always required; besides the *proportional* parts of B and C, and other *reductions* on account of the variation in the north polar distance of the star, when p and t do not correspond with the value in the table.

The observations, for determining the latitude of a place, are usually formed into series of 20 or 30 each: and if we could, by the help of any tables, annex to *each* observation its correction depending on the distance of time from the mean time of the whole series, and after dividing the sum by the whole number ($=$ N) of observations, approximate towards the correct value by the help of any formula (similar to the plan adopted by M. Delambre, in his *Reduction to the meridian*) a great deal of time and labour might be saved in the computations. But M. Littrow's formula presents no such advantages.

It is true that he has elsewhere suggested a plan of this kind, but I much doubt whether it can be practised with convenience or success. It is as follows: make

$$m = \frac{\sin p . \sin \psi}{\sin z} . \sin t$$

$$n = \frac{\sin p . \sin \psi}{\sin z} . \cos t = m . \cot t$$

$$A = \frac{2 \sin^2 \frac{\theta}{2}}{\sin 1''} + \frac{2 \sin^2 \frac{\theta'}{2}}{\sin 1''} + \frac{2 \sin^2 \frac{\theta''}{2}}{\sin 1''} + \&c.$$

where t denotes the mean time of the whole number of observations, and where θ, θ', θ'', &c. denote the difference between t and the time of each respective observation: then by assuming

$$x = (n - m^2 . \cot z) \frac{A}{N}$$

we shall have

$$\tan u = \tan p . \cos t$$

$$\cos (\psi - u) = \frac{\cos u}{\cos p} . \cos (z - x)$$

Now, this is precisely the same formula as that which I have deduced at the commencement of this paper, with the exception

tion of the new quantity x, which is here proposed to correct the value of z as deduced from a *series* of observations. But since the value of x depends on three other formulæ, none of which are of very easy arithmetical solution, it may be doubted whether any thing is gained by the substitution. It is singular that the formulæ m and n both contain the value of ψ: so that in *these* approximations also the latitude is supposed to be previously known before the general formula can be depended upon. The value of m (if formed into a table, as proposed by M. Littrow) will necessarily require a table of double entry, even for a fixed observatory. The value of A may be taken from M. Delambre's tables of *Reduction to the meridian*. M. Littrow adds, as a condition, that the observations must not be very numerous, and that if they are formed into series of four or six, we may safely reject the correction x: but, by this limitation and these subdivisions, we lose the benefit of his proposed corrections, and thus revert to the original general formula. His method indeed appears to be somewhat similar to the mode proposed by M. Soldner for determining the time from the *mean* of a number of zenith distances of the sun or a star: (see Bode's *Astronomische Jahrbuch* for 1818, page 123) but which M. Delambre (in his investigation and examination of this method, in the *Connaissance des Tems* for 1820, pages 357 and 397) seems to consider not more convenient in practice than the rigorous formula; except in those cases where the observations are made near the prime vertical.

I shall now proceed to make a few remarks on the *history* of the discovery of this problem: a measure which appears to be called for at the present moment, in consequence of some observations which have been recently made on the subject by M. Zach, in his *Correspondance Astronomique*. Till those remarks appeared M. Littrow was (I believe) generally considered as the original inventor of the method: and in the first paper which he published on the subject in 1817, he himself calls it a *new* method. M. Zach, however, in a late number of his *Cor. Ast.* (vol. vi. page 210) states that the American seamen have been a *long time* in the habit of observing the altitude of the pole-star, *at all hours of the night*, for the purpose of deducing their latitude: and he quotes a table given for this very purpose, by Mr. Bowditch in the third edition of his "New American Practical Navigator." This work was originally published in America in the year 1801; but the third edition, here alluded to, appears *without a date:* and, as it is not known whether the table was inserted in the first and second editions, no correct inference can be drawn as to the time when this method was first adopted by the American navy.

The table given by Mr. Bowditch consists merely of $p.\cos t$ converted into numbers for every 10^m, from 0^h to 12^h, and on the assumption that p is equal to 1° 42′. No correction is given for those powers of p which are omitted: and, as the polar distance of the star is assumed 4′ greater than it is at the present day, it is evident that his table can give but a very rough approximation to the latitude. The American captain, from whom M. Zach obtained the above particulars, seemed aware of this circumstance, but did not consider it of much importance, *because* the altitude of the star was seldom determinable within those limits: an error in reasoning too frequently adopted. He said, however, that if greater accuracy were required they (the Americans) had other tables more exact; and mentioned those of Capt. Elford, of Charleston in South Carolina, published under the title of "Circular Polar Tables," and a work of Mr. Stevens: neither of which he had in his possession.

Capt. Elford's work I have not yet been able to procure, neither do I know the date of it; but the pamphlet of Mr. Stevens has been kindly presented to me by Mr. Troughton; and I avail myself of this opportunity of giving a summary of its contents. It was published in this country twenty-two years ago, under the title of "A method of ascertaining the latitude in the northern hemisphere by a single altitude of the pole-star, at any time; with tables computed for that purpose. By John Stevens, in the service of the East India Company. Cambridge 1800." It contains only 16 pages; four of which are occupied by the tables. Table I. appears to contain the value of $p.\cos t$: and Table II. the correction depending on the *latitude* of the place of observation. The north polar distance of the star is assumed equal to 1° 45′; but a rule is given for correcting the errors which may arise from any variation in that quantity.

It would thus appear (from the information which we at present possess) that this method of deducing the latitude of a place originated in *this* country. But, how it has happened that it should have been so much neglected here, as not to have formed a part of any subsequent treatise on navigation, whilst it has been so speedily adopted by the Americans, I am at a loss to conceive. The attention, however, which has been recently directed to this subject, will probably restore it to its proper situation in all future works which may be written for the information of nautical persons. The *correct* solution of the problem still belongs to M. Littrow.

M. Zach, who seems to take a pleasure in noticing the zeal and rising talents of the Americans, particularly in all nautical affairs, has stated that Mr. Bowditch (in his "New American Practical Navigator") has re-calculated the logarithms, technically called *Log. Rising*, and extended them to 12^h. It may perhaps be gratifying to learn that a similar extension of those useful tables has also been made in this country by Captain Lynn in a work entitled "Solar Tables for working the longitude by chronometer, or for finding the latitude by double altitudes of the sun or stars." In the *Requisite Tables* published by Dr. Maskelyne, the *Log. Rising* are carried to five places of decimals, and for every 10 seconds of time. In those recently published by the Rev. Mr. Lax, they are extended to every 4 seconds of time, but still to 5 places of decimals only. In those now publishing by Capt. Lynn, they are extended to *every* second of time, and to *six* places of decimals. The work, as far as 9 hours, is already published, and will be equally useful to the astronomer and the seaman: but, whether it will be continued, or not, will probably depend on the patronage which is afforded to that part which is already before the public. Too great encouragement cannot be given to works of this kind, which abridge so much the labour of computation.

In deducing the time from altitudes of the sun or stars, by means of the strict analytical formula, we obtain $\sin^2 \frac{t}{2}$ as the value of the hour angle. This expression is of frequent occurrence in various astronomical solutions, and had induced me to form a table of its value for every *second* of the quadrant. By the help of Capt. Lynn's tables, however, this labour might have been saved, since we have merely to add the constant logarithm 5.3010300 to the logarithm of $\sin^2 \frac{t}{2}$, and we immediately obtain the *Log. Rising*: which, as I have already observed, is calculated in Capt. Lynn's tables to every second; and which consequently shows, upon inspection, the correct time required, without any further reduction. Its application would have been more general if it had contained the values of the angles in *arc* as well as in *time*; similar to the plan adopted by Mr. Lax.

VIII. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1822, *at the Time of passing the Meridian of Greenwich. By the Rev.* J. GROOBY.

[Continued from vol. lix. page 101.]

The Author wishes to remark that the last six months of the year have not been calculated from the same tables as the former, but from more recent ones published in the Konigsburg Observations for 1818. The mean right ascension as there given by Bessel has also been used, instead of that given by Mr. Pond.

1822	γ Pegasi	α Arietis	α Ceti.	Aldebaran	Capellæ	Rigel	β Tauri	α Orionis.	Sirius	Castor	Procyon	Pollux	α Hydræ	Regulus	β Leonis	β Virginis	Spica Virginis	Arcturus.
	H M 0 4	H M 1 57	H M 2 53	H M 4 25	H. M. 5 3	H. M 5 6	H M 5 15	H M 5 45	H. M 6 37	H M 7 23	H M 7 29	H. M 7 34	H M 9 18	H M 9 58	H M 11 40	H M. 11 41	H M 13 15	H M 14 7
July	s	s	s	s.	s.	s.	s	s	s	s	s	s	s.	s	s	s	s	s
1	7 31	11 48	0 69	44·27	34 61	0·05	4 10	53 23	18 58	14 72	59 76	25 89	51 31	54 47	0 44	27 45	52 15	35 46
2	34	51	72	30	64	07	12	24	59	73	77	90	30	47	43	44	14	45
3	37	55	75	33	67	09	15	26	60	74	77	90	30	47	42	44	13	44
4	40	58	78	35	70	11	17	28	61	75	78	91	30	47	41	43	12	43
5	44	61	81	38	73	13	20	30	63	76	79	92	30	46	40	42	11	42
6	47	65	84	40	76	15	22	31	64	77	80	93	30	46	39	41	10	41
7	50	68	87	43	79	17	24	33	65	78	81	94	30	46	38	40	09	40
8	53	71	90	45	82	19	27	35	66	79	82	95	30	45	37	40	08	39
9	57	75	93	48	85	21	29	37	68	80	83	96	30	45	37	39	07	38
10	60	78	96	51	88	23	32	39	69	82	84	98	30	45	36	38	06	37
11	63	82	99	54	92	25	34	41	70	83	85	99	30	45	35	37	05	36
12	66	85	1 02	56	95	28	37	43	72	85	86	26 01	30	44	34	36	04	35
13	69	89	05	59	98	30	40	45	73	86	87	02	30	44	33	36	03	34
14	72	92	08	62	35 02	32	42	47	75	88	89	04	30	44	32	35	02	32
15	76	96	11	65	05	35	45	49	76	90	90	05	30	44	32	34	01	30
16	79	99	14	68	09	37	48	51	78	91	91	07	31	44	31	33	51 00	29
17	82	12 02	17	70	11	39	50	53	79	93	92	08	31	44	30	32	99	28
18	85	05	20	73	16	42	53	56	80	95	94	09	31	44	29	32	98	27
19	88	09	23	76	19	44	56	58	82	97	95	11	31	44	29	31	97	26
20	91	13	26	79	23	47	58	60	84	98	97	13	31	44	28	30	96	25
21	94	16	29	82	27	49	61	63	85	15 00	98	14	32	44	28	29	95	23
22	96	20	32	85	31	52	64	65	87	02	99	16	32	44	27	29	94	21
23	99	23	36	88	35	54	67	67	89	04	60 01	17	32	44	26	28	93	20
24	8 02	26	39	91	39	57	70	70	91	06	02	19	32	44	26	28	92	19
25	05	30	42	94	43	59	73	72	93	08	03	21	33	44	25	27	91	17
26	07	33	45	97	47	62	76	75	94	09	05	22	33	44	25	26	90	16
27	10	36	48	45 00	51	64	79	77	96	11	06	24	33	45	24	26	89	15
28	13	40	51	03	55	67	82	79	98	13	07	26	34	45	23	25	88	14
29	16	43	55	06	59	70	85	82	19 00	15	09	28	34	45	23	25	87	13
30	18	46	58	09	63	72	88	84	02	17	11	30	3	45	22	24	86	11
31	21	49	61	12	66	75	91	87	04	19	12	32	35	45	22	24–23	85–84	10–09

1822.	1α Libræ	2α Libræ	α Cor Bor	α Serpentis	Antares	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ	α Aquilæ.	β Aquila.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H M 14 40	H. M. 14 41	H M 15 27	H. M 15 35	H M 16 18	H M 17 6	H. M 17 26	H M 18 30	H M. 19 37	H. M. 19 42	H M. 19 46	H. M 20 7	H. M 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 17	H. M. 22 55	H. M. 23 59
July	s.	s	s	s	s.	s	s	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	54 65	6 07	12 34	35 63	34 49	35 46	43 87	57 96	51 25	9 31	37 66	50 35	14 12	24 88	41·52	51 17	56 81	14·69
2	64	06	33	62	49	46	87	96	27	33	68	37	14	90	55	20	84	72
3	64	06	32	62	49	46	88	97	28	34	69	39	16	92	57	23	87	75
4	63	05	31	61	48	46	88	97	30	36	71	41	18	94	60	27	90	79
5	62	04	30	61	48	46	88	97	32	37	73	43	20	96	63	30	93	82
6	62	04	30	60	48	46	89	98	33	39	74	45	22	98	65	33	96	85
7	61	03	29	60	48	46	89	98	34	40	76	47	24	25 00	68	37	99	88
8	61	03	28	59	48	46	89	99	35	42	77	49	26	02	70	40	57·02	91
9	60	02	27	59	48	46	89	99	36	43	79	51	28	04	73	43	05	95
10	59	01	26	59	47	45	89	99	37	44	80	53	30	05	75	46	08	98
11	58	00	25	58	47	45	89	99	38	45	82	54	31	07	78	49	10	15 01
12	58	00	24	57	47	45	89	99	39	46	83	56	33	08	80	52	13	04
13	57	5 99	23	56	46	45	89	99	40	48	84	57	34	10	82	55	16	08
14	56	98	22	56	46	44	89	99	41	49	85	59	36	11	85	58	19	11
15	55	97	21	55	46	44	89	99	42	50	87	60	37	13	87	61	21	14
16	54	96	20	54	45	44	88	99	43	51	88	62	39	14	89	64	24	17
17	53	95	19	53	45	43	88	99	44	52	89	63	40	16	91	67	27	20
18	52	94	18	53	44	43	88	99	45	53	90	65	42	17	94	70	29	23
19	51	93	16	52	44	43	88	99	47	54	91	66	43	19	96	73	32	27
20	50	92	15	51	43	42	87	98	48	55	92	67	44	20	98	76	34	30
21	49	91	13	50	42	42	87	98	48	55	92	68	45	21	42 00	78	37	33
22	48	90	12	49	42	41	87	97	49	56	93	69	46	22	02	81	39	36
23	47	89	10	48	41	40	86	97	49	56	94	70	47	23	04	84	42	39
24	46	88	09	47	40	40	86	96	50	57	95	71	48	24	06	87	44	42
25	45	87	08	46	39	39	85	96	51	58	95	72	49	25	08	89	46	45
26	44	86	06	45	38	38	85	95	51	58	96	73	50	26	10	92	49	48
27	43	85	05	44	38	37	84	95	52	59	97	74	51	27	12	95	51	51
28	42	84	03	43	37	37	84	94	53	59	97	75	52	28	14	97	54	54
29	40	82	02	41	36	36	83	94	53	60	98	76	53	28	15	52 00	56	57
30	39	81	01	40	35	35	82	93	53	60	98	77	54	29	17	02	58	60
31	38	80	11 99	39	34	34	82	92	53	61	99	77	54	29	18	04	60	62

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 29	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Aug.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	8·23	12·52	1·64	45·15	35·70	0·77	4·94	33·90	19·06	15·21	60·14	26·34	51·36	54.45	0·21	27·23	51·83	35·07
2	26	56	67	18	73	80	97	92	08	24	16	36	37	45	21	22	82	06
3	28	59	70	21	77	83	5·01	95	10	26	18	38	37	46	20	22	81	05
4	31	62	73	24	81	85	04	97	12	28	20	40	38	46	20	21	80	04
5	33	66	76	27	85	88	07	34·00	14	30	21	42	39	46	19	21	79	03
6	36	69	79	30	88	91	10	02	16	32	23	44	39	47	19	21	78	02
7	39	72	82	33	92	94	13	05	18	35	25	46	40	47	18	20	77	00
8	42	75	86	37	96	97	17	08	21	37	27	48	41	48	18	20	76	34·99
9	44	78	89	40	36 00	99	20	11	23	40	29	50	42	49	17	20	75	97
10	46	81	92	43	04	1·02	23	13	25	42	31	53	* 42	49	17	19	74	96
11	48	84	95	46	08	05	26	16	28	44	33	55	44	50	16	19	73	94
12	51	87	98	50	12	08	30	19	30	47	35	57	45	51	16	19	72	93
13	53	91	2·01	53	16	10	33	21	32	49	37	59	46	51	16	18	71	92
14	55	94	04	56	20	13	36	24	35	52	39	61	47	52	15	18	70	91
15	57	97	07	59	24	16	40	27	37	54	41	63	48	53	15	18	69	89
16	59	13·00	10	62	28	19	43	29	39	57	43	65	49	53	15	18	68	88
17	62	03	13	65	32	22	46	32	42	59	45	68	50	54	14	17	67	86
18	64	06	16	69	37	25	50	35	44	62	47	70	51	55	14	17	66	85
19	66	09	19	72	41	28	53	38	46	65	49	73	52	56	14	17	65	83
20	68	12	22	75	44	30	56	41	49	67	51	75	53	* 58	14	17	64	82
21	70	15	24	78	48	33	60	43	51	70	54	78	55	59	14	17	63	81
22	71	18	27	81	52	36	63	46	55	73	56	80	56	59	14	16	62	80
23	73	20	30	84	56	39	66	49	56	75	58	83	57	60	14	16	61	78
24	75	23	33	87	61	42	69	52	59	78	60	86	58	61	13	16	61	77
25	77	26	36	90	65	45	73	55	62	81	62	88	59	62	13	16	60	76
26	79	29	39	93	69	48	76	57	64	83	64	90	60	63	13	16	59	75
27	81	32	42	97	73	51	79	60	67	86	67	93	62	64	13	16	58	73
28	83	34	45	46·00	79	54	82	63	70	89	69	96	63	65	13	16	58	72
29	84	37	48	04	83	57	85	66	72	92	71	99	65	66	13	16	57	71
30	86	39	50	06	87	60	89	69	75	95	74	27·01	66	67	13	16	57	70
31	87	42	53	09	91	63	92	72	77	98	76	04	68	68	13	16	56	69

1822.	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri.	2 α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Aug.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	54·36	5·78	11·97	33·38	34·33	35·33	43·81	57·92	51·53	9·61	37·99	50·78	14·55	25·30	42·20	52·06	57·62	15·65
2	35	77	96	37	32	32	80	91	53	61	99	78	55	30	21	09	64	67
3	34	76	94	35	31	31	79	90	53	62	99	79	56	30	23	11	66	70
4	33	75	92	34	30	30	78	89	53	62	99	80	57	30	24	13	68	73
5	31	73	91	33	29	29	77	88	53	62	99	80	57	30	26	15	70	75
6	30	72	89	32	28	28	76	87	54	62	38·00	81	58	31	27	18	72	78
7	29	71	88	31	27	27	75	86	54	63	00	82	59	31	28	20	74	80
8	28	70	86	29	25	26	74	85	54	63	00	82	59	31	29	22	76	83
9	26	68	84	28	24	25	73	83	54	62	00	83	60	31	30	24	77	85
10	25	67	83	26	23	23	71	82	53	62	00	83	60	30	32	26	79	88
11	24	66	81	25	21	22	70	80	53	62	00	83	60	30	33	27	80	90
12	23	65	79	24	20	21	69	79	53	62	00	83	60	30	34	29	82	92
13	22	64	78	23	18	19	68	77	53	62	00	83	60	30	35	31	83	95
14	20	62	76	21	17	18	67	76	53	62	37·99	84	61	30	36	33	84	97
15	19	61	74	20	15	17	66	74	53	62	99	84	61	29	37	35	86	99
16	18	60	72	19	14	15	64	73	52	61	99	84	61	29	38	36	87	16·01
17	17	59	71	17	12	14	63	71	52	61	99	84	61	29	39	38	89	04
18	15	57	69	16	10	13	62	70	52	61	99	84	61	28	40	40	91	06
19	14	56	67	14	09	11	61	68	51	60	98	84	61	27	41	41	92	08
20	13	55	65	13	07	10	59	66	51	60	98	83	60	27	42	43	94	11
21	12	54	64	11	08	08	58	65	50	59	97	83	60	26	42	44	93	13
22	10	52	62	10	06	07	56	63	49	59	97	83	60	25	43	46	94	15
23	09	51	60	08	05	05	55	61	49	58	96	83	60	24	44	47	96	17
24	08	50	58	07	03	04	54	59	48	57	96	82	59	24	45	49	97	19
25	06	48	57	05	02	02	52	57	48	57	95	82	59	23	46	50	98	21
26	05	47	55	04	00	01	51	56	47	56	95	82	59	23	46	52	99	23
27	04	46	53	03	35·98	99	49	54	46	56	94	82	59	22	47	53	58·01	25
28	02	44	51	01	96	98	48	52	45	55	93	81	58	21	47	54	03	27
29	01	43	49	00	94	96	46	50	44	54	92	80	57	20	48	55	04	28
30	00	42	48	32·98	93	95	44	48	43	53	92	80	57	19	48	56	05	30
31	53·98	40	46	97	91	93	43	46	42	52	91	79	56	18				

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Sept.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	8·89	13·45	2·56	46·12	36·95	1·66	5·96	34·75	19·80	16·01	0·79	27·07	51·69	54·69	0·14	27·16	51·55	34·68
2	90	47	58	15	99	69	99	78	83	04	81	09	71	71	14	17	55	66
3	92	50	61	19	37·04	72	6·03	81	85	07	83	12	72	72	14	17	54	65
4	93	52	64	22	08	75	06	84	87	10	86	15	74	73	14	17	53	64
5	94	55	66	25	12	78	10	87	91	13	88	17	75	74	14	17	52	63
6	96	57	69	28	17	81	13	90	94	16	91	20	77	75	15	18	52	62
7	97	60	72	31	21	84	17	93	97	19	93	23	78	77	15	18	51	60
8	98	62	75	34	25	86	21	96	99	22	96	26	80	78	15	18	51	59
9	99	65	77	37	29	89	24	99	20·02	25	98	29	82	80	15	19	50	58
10	9·00	67	79	40	33	92	27	35·02	05	28	1·01	32	84	81	16	19	50	57
11	01	69	82	43	38	95	31	05	07	31	04	35	86	83	16	20	49	56
12	03	71	84	46	42	98	34	08	10	34	06	38	87	85	16	20	49	54
13	04	74	87	49	46	2·01	38	11	13	38	09	41	89	86	17	20	48	53
14	05	76	89	52	50	04	41	14	16	41	12	44	91	88	17	21	48	52
15	06	78	92	55	54	07	45	17	19	44	14	47	93	89	17	21	47	51
16	07	81	94	59	59	10	48	20	22	47	17	50	95	91	18	22	47	50
17	08	83	97	62	63	13	52	23	25	51	20	53	97	93	* 19	22	46	49
18	09	85	99	65	67	15	55	26	27	54	22	56	99	94	20	* 23	46	49
19	10	87	3·01	68	71	18	58	29	30	57	25	59	52·01	96	20	24	46	48
20	10	89	03	71	75	21	62	32	33	60	28	62	03	98	21	25	46	47
21	11	91	06	74	79	24	65	35	36	64	31	66	05	55·00	22	25	45	46
22	12	93	08	76	83	27	68	38	39	67	34	69	07	02	23	26	45	45
23	12	95	10	79	88	29	72	41	42	70	36	72	10	04	24	27	45	45
24	13	96	12	82	92	32	75	44	45	74	39	75	12	05	25	28	45	44
25	14	98	15	85	96	35	78	47	48	77	42	78	14	07	26	29	45	43
26	14	14·00	17	88	38·00	38	82	50	51	80	45	81	16	09	27	30	45	43
27	15	02	19	91	05	41	85	53	54	84	48	85	18	11	28	30	45	42
28	15	04	21	94	09	44	89	56	57	87	50	88	20	13	29	31	46	41
29	16	05	23	97	13	47	92	59	60	91	53	91	23	15	30	32	46	41
30	16	07	25	99	17	49	95	62	63	94	56	94	25	18	31	33	46	40

1822.	1α Libræ.	2α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni.	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Sept.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	53·97	5·39	11·44	32.96	89	34·91	43·42	57·44	51·41	9·51	37.90	50·78	14·55	25·17	42·49	·56	58·06	16·31
2	96	38	42	94	88	89	41	42	40	50	89	77	54	16	49	57	07	33
3	94	36	40	93	86	88	39	40	39	49	88	77	54	14	49	58	08	34
4	93	35	39	91	84	86	38	38	38	48	87	76	53	13	50	59	09	36
5	92	34	37	89	83	84	36	36	37	47	86	75	52	12	50	60	10	37
6	80	32	35	87	81	83	34	34	36	46	85	75	52	11	50	61	11	39
7	89	31	33	86	79	81	32	32	35	45	84	74	51	09	50	62	12	40
8	88	30	31	85	77	79	30	30	34	44	83	73	50	07	50	62	12	42
9	87	29	30	83	76	77	28	27	32	43	82	72	49	06	50	63	13·13	43
10	86	28	28	82	74	76	27	25	31	42	81	71	48	04	50	63	13	44
11	84	26	26	80	72	74	25	22	30	41	80	70	47	02	50	63	14	45
12	83	25	25	79	70	72	23	20	28	39	78	69	46	00	50	64	14	46
13	82	24	23	78	69	70	21	18	27	38	77	68	45	24·99	49	64	14	48
14	81	23	21	76	67	68	19	15	26	37	76	67	44	97	49	64	15	49
15	80	22	19	75	65	67	18	13	25	36	75	66	43	95	49	64	15	50
16	79	21	18	73	64	65	16	10	23	34	74	65	42	94	49	64	15	51
17	78	20	16	72	62	63	14	08	22	33	72	64	41	92	49	65	16	52
18	77	19	15	71	61	61	12	06	20	31	71	63	40	90	48	65	16	53
19	76	18	13	69	59	60	10	03	19	30	69	61	38	88	48	65	16	53
20	75	17	12	68	58	58	09	01	17	28	68	60	37	86	47	65	16	54
21	74	16	10	67	57	56	07	56·98	16	27	66	58	35	84	47	65	16	55
22	73	15	09	66	55	54	05	96	14	25	65	57	34	82	46	65	16	56
23	72	14	07	64	54	53	03	93	13	24	64	56	33	80	45	64	16	56
24	71	13	06	63	53	51	01	91	11	22	62	55	32	78	45	64	16	57
25	70	12	04	62	52	49	42·00	88	10	21	61	54	31	76	44	64	16	58
26	69	11	03	60	50	48	98	86	08	19	59	52	29	74	44	64	16	58
27	69	11	01	59	49	46	96	83	07	18	58	51	28	72	43	64	16	59
28	68	10	00	58	48	44	94	81	05	16	56	49	26	70	42	63	15	59
29	68	10	10·99	57	46	43	93	78	04	15	55	48	25	67	41	63	15	60
30	67	09	97	56	45	41	91	76	02	13	53	46	23	65	41	62	15	60

IX. *On the Pollen of Flowers.* *By Mrs.* Agnes Ibbetson.

To Dr. Tilloch.

Sir, — My last gave the most convincing proof that there is no such thing as perspiration in plants, and that the water found under a glass when a vegetable is placed within it (without earth) is nothing more than the condensation of the atmosphere, shown and exemplified by the sky-light, and the water that runs on the stairs; and there being scarce a drop of moisture in the interior vessel when the plant is covered with two glasses, though the exterior cylinder is inundated as usual.

I now turn to my next subject, which will give the whole history of the pollen from the moment it is first protruded in the plant to the time it completes its fructification of the seed. The pollen is always formed in the tap root (Pl. I. fig. 1, *a a*): the anthers and filaments are a mere late production, and are never discovered in the plant till the bud forms them while completing other parts at the top of the plant: the pollen is therefore formed in the root perfectly uncovered (Pl. I. fig. 1, *a a a*): if the tap root is eradicated, the pollen forms just above the cut; but is rarely in trees found in the side roots, except in *firs.* It passes up in all trees but in the Dioecious order, where, being separated entirely from the female, it runs up the pith or interior wood of the stem, in the male plant perfectly naked, fig. 11, *m.* When the anthers begin to form in the male bud, in dioecious trees, nothing can be more evident than the entering of the pollen into them, fig. 2. They are then so stuffed and filled to bursting as to form a curious moving specimen for many minutes after being placed under the glass. It is then that the filaments are formed and shoot forth from the male bud, that they may carry the anthers and pollen into the air, where they generally swing for some hours, if not days. There is every reason to believe that they thus procure a quantity of gas, absorbing it by the help of their motion: thus they soon become ripe; and when the pistil has displayed those indications of being ready to complete the *seed,* by the swelling of the bubble of liquid at the summit, the pollen again leaves the anthers and flies to that nectarious drop, which attracts it to the pistil, and remains fastened by the hairs to which the balls adhere, and are in a short time dissolved by the juices of the pistil, down which it runs in various gutters till it has reached the seeds, which it enters mixed with the nectarious juice, and thus fructifies them.

When the pollen passes up in Dioecious trees, it forms a curious specimen: the balls of the pollen are generally large and run up the pith in different short vessels, figs. 1 and 2. I know not a better example of Dioecious trees than the Willow: they are all the same in their flowering; two stamens alone appear out of the bud, from which they are classed, fig. 1.; the rest pass from the male bud to the female, and fructify all the seeds: fig. 4 is the nectary of the male bud of the Willow, fig. 3 ×, of the female. It will be seen that the male is led up by the spiral wire, the female by the alburnum; and so invariable is this law, that it passes through every class of plants, and serves to distinguish the sexes in all the *Cryptogamia* plants, where such a mark is of consequence, and I never saw it vary in any one instance.

When the plants are Monoecious, and have the male and female in the same tree, you may always see, at the termination of the piece of stem, to which sex that piece belongs, and in herbaceous plants, such as the Indian Wheat, Carexes. The female first surrounds the male; they then cross each other, and the female is discovered fixed in the centre of the plant, to form the cone; in cutting the plant open all the way, the parts between the knots are always filled by the style of the female plant, while the seeds are growing under the knots: then the male flower passes by the female and makes its appearance at the top of the grass or corn.

It is most evident why the pollen comes from the tap root naked, or without anther or filament: a great quantity must pass up to the bud, and through the new shoot; the vessels are small, and would not contain either addition, especially the anthers, which are so formed with their four corners that they could not pass through a straight vessel, fig. 5. I was a long time before I discovered this. It is curious not only to see the loads of pollen that run up the new shoot, but the early time they begin to run up. The stem, the moment the new shoot is beginning to protrude, and the pollen mounts it, is the first process for the new year, which in plants may be said to begin in August. I have often seen the pollen waiting in vast quantities near the new screw of the Poplar a month before the shoot was completed; then the moment they can rise and pass the screw, fig. 6, they are so loaded with the balls, so crowded together, that it is wonderful with what ease they pass up. It is admirable that every important part is formed in the root, and not one that is not *absolutely essential*, and of real consequence to the fructification:—the female flower-bud, for example, but not the leaf-bud; the pollen, but not the anthers and filament; the heart of the seed, but not the seed itself, which, large as it is, is a mere appendage and cover to the heart, which contains that shoot which is to form the next year's plant or tree.

The pollen, though it appears so small, is far from being so in Dioecious trees; and though they seem all like balls gathered in a heap, yet when examined by the microscope, they differ extremely in shape: some are round, some square, some triangular, some resemble a thick round cake; nor are they less various in colour, for though most of them are yellow, yet there are many white, purple, blue, and red; but most pollen belonging to the same genera have the same form and the same shape, which is an easy way of discovering the class of the flower. If the stamens are taken out, and the pollen is shaken on a dry glass, and a drop of water then allowed to fall near it, in a moment each ball of pollen (though before so apparently agglutinated) is scattered as if struck with electricity and flies to every part of the glass. They must contain much sulphureous matter, for they will often burn at the candle; but they do not melt in hot water, nor even in boiling spirit of wine, which will however sometimes draw from them a faint tint of colour; but it does not dissolve the *power*, though the pollen powder is directly melted by the nectarious juice on the pistil. Most probably it is resinous. They have invariably (let their shape differ ever so much) two separate parts; and two different liquids. The outward part (as at fig. 5) is formed of a slight cuticle, sometimes crossed, or with circular apertures, which show the dark parts within, and which often exhibit vessels shooting from the exterior ball, fig. 5 *a a* ×, and ejecting oil; but when the inner ball bursts, it is discovered to be formed of a cartilaginous matter extremely strong; but all the apertures proceed from the upper surface alone and ejecting oil. But when the inner ball explodes, a thin vapour pours from it like undulating smoke, fig. 6 ×, and spotted with grains of black.

Rumphius and Hedwig, another German botanist, who had studied the subject as well as the microscopes then permitted, differed much: one of them said it was all oil, fig. 7, which proceeded from the pollen ball. His antagonist insisted it was a gray liquid studded with black spots; they certainly were *both right* and *both wrong*; since the mixture of the two opinions really composes the pollen ball, being oil without, and gray matter within. They are formed of such a beautiful matter, and the apertures are so contrived, as to throw a kind of luminous appearance within by means of a shining matter, which often produces a curious effect when contrasted with the black of the inner box: this adds not a little to those velvet flowers, such as the Heart-ease, &c. The curious round ball of the Cucumber and Melon are so remarkable for their beauty, the outward figure is so light and highly fashioned, that it is difficult to describe it; see fig. 8, *a a*..

But it is not anthers and pollen alone that form this necessary conjunction. The filament is of no little consequence: the thread which shoots from the anther after that is filled with pollen (thin as it is) must be perfectly hollow the whole way, fig. 9 *a a*, and fig. 9; for so entirely does every ingredient, every nutriment, proceed from the root, that all must have free access to that part which apertains to it, and is intended to rise to its surface,—the upper part of the flower and the pistil. The anthers are and may be described as little bags of exquisite workmanship, each *sack* divided into two or four partitions, but always extremely filled by the pollen: when developed, the opening of these cases is most curiously managed with an art and cleverness nature alone can produce; since the more they are magnified the more beautiful they appear, which is never the case with the works of art. Some of the anthers open at the end, fig. 9 *c c*; some all the way down the side, fig. 9 *b b*. Some, still more extraordinary, are circular with a bar between each pollen ball, in order to ensure a passage thoroughly free through each part, when the filaments that serve as a foot-stalk to the anthers should possess a free communication. When the filaments that support them grow, they are formed perfectly hollow, that the dust may be changed should the moisture injure it. Sometimes the threads enlarge near the bottom to admit a *nectary*; at other times above. It is generally contrived with excessive art, as in the *Antirrhinum*, where the stalk is twisted into a spiral before it joins the anther. This would appear to prevent the passage of the pollen; but I have exactly watched the flower; this spiral does not show itself till after the anther is completely stuffed with pollen: some anthers are formed like a divided heart, fig. 10 *c c*; some resemble a flower, *d d*. If communication were not necessary between the flower, and thus down to the root, there would have been no occasion for a cylinder reaching through the filament; whereas in the many thousands I have dissected I never found this part missing, and very often full of pollen: then the anthers have a hair peculiar to themselves which almost always conveys a coloured juice: progressive dissection soon proves these important truths, and unites them all, and gives a further insight into botanical nature, than years of more desultory studies on the same subject. Indeed it is the only way of becoming really acquainted with the interior of a tree.

To understand the stamen perfectly, there needs only to dissect a Dioecious tree or plant; there needs only to follow the picture the year round, as I have done, and see *that* pass *within* the tree, which is afterwards displayed *without*. In the Willow it is so entirely divided, the *male from the female*, that it is impossible to mistake them: the male Willow tree sends most of its pollen through the wood and pith by means of short vessels, which I have already shown: but it is very difficult to draw the pollen running within the anthers; though take a young flower of the male kind, and divide it, and it will be viewed directly. I hardly know a specimen plainer or more easily shown in the original, fig. 2. But there is one peculiarity which distinguishes the Dioecious;—the male plant is led up by the spiral wire, fig. 13, the female by the line of life, fig. 13 ×: this is so universally the case, that in all plants of this order, particularly the *Cryptogamia*, it is this that points out the male plant: I have dissected an immense number to trace the male and female flower, and invariably found this vessel, and the male is the more easily discovered from its always possessing so much motion; for in some plants it is absolutely violent, as in most of the *Cryptogamia*:—but I shall give a letter on this subject. Your obliged servant,

AGNES IBBETSON.

Description of the Plate.

Fig. 1. Male flower of the Willow.
Fig. 2. Male flower of the Willow at an earlier stage, when the pollen are entering the anthers at *a a a a*.
Fig. 1 × *a a*. The tap root of the anther, with the first growth of the pollen.
Fig. 3, of the female flower of the Willow, with the heart of the seeds entering the seed-vessel.
Fig. 4. Pointing out the nectary in the male *d d d*, and the length of the nectary in the female flower.
Fig. 5. The exact interior shape of an oblong pollen, to show how the interior box is made, and how the exterior is formed to eject the oil.
Fig. 5 *a a a* ×. A pollen ball: × the passage for the oil it ejects; and if some water swims on the glass it will be constantly seen to throw out a sort of pattern of oil.
Fig. 6. The sort of screw which stops the new shoot.
Fig. 6 ×. The ball of the Melon or Gourd, with the interior manner of ejecting the gray matter.
Fig. 7. Pollen ball.
Fig. 8 *a a*. Triangular ball with the oil vessels *a a* completed.
Fig. 9. The filaments: fig. 9 *a a a*, to show they are hollow.
Fig. 9 *f f*. The filaments.
Fig. 10 *c c*. Double heart.
Fig. 11. Stem, with the pollen passing up.

X. *On the Heat produced by Chlorine, and on a singular Effect produced by Lightning.* *By* John Murray, *F.L.S. M.W.S. &c. &c.*

To Dr. Tilloch.

Sir,—Dr. Hare, of Philadelphia, is stated to have found that though the thermometer immersed into a volume of chlorine gas is not affected, the sensation of heat on the skin announces it to be 90° to 100° F. This however I believe only takes place on opening the vessel in contact with atmospheric air.

This phænomenon is very ingeniously, and I believe justly, ascribed to a chemical action between the gas and the insensible perspiration of the skin.

Allow me to add that the same phænomena are manifested when a volume of nitrous gas is opened into the atmosphere: the thermometer rises only a fraction, but the sensation of heat on the hand is equal to that superinduced by chlorine.

My feelings deceive me if the same thing does not occur in the case of muriatic acid gas: the quantity which I collected over the mercurial cistern was contained in a small cylinder, and would not admit the immersion of my whole hand.

I have been much interested in reading an account of "a singular effect produced by a stroke of lightning on the 3d July last, communicated by Professor Pictet to the Helvetic Society." The lightning is represented as having done no damage to the house, but "it perforated a piece of white iron with *two holes* of an inch in diameter, and five inches distant; and, what was very remarkable, the *burs at the edges of the holes were in opposite directions.*" Professor Pictet draws the conclusion, that either two electric currents had moved simultaneously, and in opposite directions, or that, after it had perforated the metal and moving five inches along it, had penetrated the plate in an opposite direction. I must confess, however, that *to me* the latter opinion seems to violate the principles of probability.

You may remember, sir, that some years ago you were good enough to insert in the Philosophical Magazine a communication of mine which detailed experiments *precisely similar* in the phænomena presented.

It may not be irrelevant to advert to two of them. A wire of the thickness of a knitting-needle rested on the table, one end being in contact with the exterior coating of the charged Leyden jar, a card rested on the wire, and a patch of tinfoil was placed over the further end of this wire: another card succeeded, and a similar patch of tinfoil with a card and wire above, terminated the arrangement. The patches of tinfoil, and of course the ends of the wires which terminated below and above them, were separated from each other about two inches. When the electric discharge was made by placing one of the balls of the insulated discharging rod in contact with the further end of the upper wire, &c. the under patch of tinfoil exhibited an *indent upward*; and the upper patch an *indent downward:* they were frequently perforated, and the burs similarly directed. If I used *two wires below*, parallel to each other, say ¾ths inch apart, and *one wire above*, then had I two indents or two burs directed upwards, and one indent or one bur downwards; and on the other hand, when I employed *two wires above and one below*, the phænomena were reversed, there being *two burs or indents* from *above*, and *one only* from *below*.

In this detail there is described another phænomenon not less interesting. I coated uniformly with *China ink* the ball of the rod connected with the internal metallic surface of the Leyden jar, as well as that of one of the balls of the jointed discharger, and placed a vertical card between these balls. When I made the discharge, the card exhibited the usual appearance of a bur raised on both sides. But independently of this, I found a *circular portion of the China ink displaced from both of these balls, and an indent in each of them.* Each ball was *an inch distant* from the respective surfaces of the interposed card, and the discharge was a powerful one. It was on these that I founded the opinion that Mons. Moll's experiment was only a modification of mine; and I have transmitted to Professor Moll of Utrecht a note of these experiments, agreeably to his request to me.

It is highly satisfactory for me to receive, after the lapse of a few years, the verification of these experiments, sealed as it were by the signet of celestial fire.

I have the honour to be, sir,
Your obliged and humble servant,
May 15, 1822. J. Murray.

XI. *A Reply to Mr.* John Murray, "*on the Apparatus for restoring the Action of the Lungs in apparent Death.*" *By Mr.* John Moore, *Jun.*

To Dr. Tilloch.

Sir, — Is it not singular that Mr. Murray should have been so displeased with me, for the notice I took of his machine for producing artificial respiration, when he has given an invitation to the public to completely investigate it; requesting them to show objections, if any were to be found? He says, "I confess that I am not a little surprised at a communication by Mr. John Moore junior, in your Number for March last: with sufficient self-complacency this correspondent considers the plan he proposes for restoring the action of the lungs as more complete than my invention." I would ask Mr. J. Murray what was the reason he stopped at his invention; was it because, by not publishing the reason I gave for my conclusions, he thought to gain a trifling advantage? He writes, I consider my plan the best, "with sufficient self-complacency:" ought he to be angry because I felt a pleasure in what I then was writing, when the same feelings, I have no doubt, induced him to publish his invention?

Mr. Murray says that "Mr. John Moore junior is pleased to adopt the form of the syringe which I had done long before; aye, and constructed and published too." I deny having adopted the form of Mr. Murray's syringe; for my plan was shown to some of my friends some years ago: but my first letter to one of them in London, relating to the apparatus, is dated February 1819. His favour to me in reply, 9th October 1819, which is about two years before I saw Mr. Murray's description.

Mr. Murray writes, "I never believed myself infallible, or that my invention was incapable of improvement. I hope I am not so absurd or unreasonable: but I do *fearlessly assert*, that his improvement, as he *insinuates* it to be, is one which adds to the complexity of the mechanism without subserving its utility; nay, rather injures the cause it is meant to serve."

What proof is there in a "fearless assertion?" Why did he not give unequivocal proof that it was incompetent to perform that which I had stated it would? This would have been much better than a "fearless assertion." How can he conclude that I "insinuated?" I said that I considered my plan more complete than his, stating my reason for the conclusion: but if I had been a dealer in positive assertions, I should have said mine was superior to his.

Mr. Murray says, "Various plans presented themselves to my mind before I completed my improved apparatus: a structure *somewhat* similar to the one now set forth and *vaunted* by your correspondent John Moore junior, was immediately rejected from its complete uselessness." This proves nothing, as he has *only asserted* it was *somewhat similar*; nor does he tell us in what particular it was dissimilar. Was it not in some essential part? or will he '*fearlessly assert*' that mine is completely useless?

If Mr. Murray had left out the word "vaunted," it would have been understood as well, and quite as correct.

Mr. Murray says that his machine was 'immediately rejected,' because, until natural respiration returns, the air undergoes no change whatever. If it be as Mr. Murray has stated, that the air undergoes no change whatever, how is it that the individual becomes reanimated? He says it is absurd to give a continued supply of fresh air to an individual, until the natural respiration returns. Will Mr. Murray say that the natural respiration returns fully and at once? because, if it returns by degrees, will not the air be decomposed in proportion to returning life? And as the air becomes charged with carbon as the person continues reanimating, does it not follow that it should be withdrawn, and fresh supplied in its place, unless Mr. Murray can show that fresh air would injure or prevent resuscitation; or that the 'victim' it would keep in 'inglorious repose?'

Mr. Murray states, "As to the application of the instrument to the purposes of a gas blow-pipe, and the exhibitions of nitrous oxide, I can have no ambition to claim an interest in such an association; *the transition* from the resuscitation of human beings to a gas blow-pipe, &c. is so entirely ludicrous, that I am astonished such an erratic fancy should be indulged in." Here he has wrong stated my expressions; but even suppose they were as he has stated them, would any man in his senses think of ridiculing *the mentioning* in immediate succession the several uses of a machine? Perhaps Mr. Murray was indulging in an 'erratic fancy,' or would he not have observed, that I was describing some mechanism, not the resuscitation of human beings as he has asserted?

The remainder of his motley communication does not relate to me, although he has headed it "On *the* apparatus for restoring the action of the lungs in apparent death."

But the charge, that I endeavoured to detract from the merits of his invention, requires for answer, that I have not so done; I said that I considered my plan more complete than his, and gave my reasons for it.

In conclusion, I shall merely say, that should Mr. Murray describe an instrument more complete than mine, I shall not be angry with him for it; for I consider the advancement of humanity and science far superior to individual feeling.

I remain, sir, respectfully yours, &c.

Lawrence Hill, June 20, 1822. JOHN MOORE Junior.

XII. *On*

XII. *On the Chemical Composition of white efflorescent Pyrites.* *By* M. J. BERZELIUS*.

It is well known that the crystalline form of white pyrites differs so essentially from that of yellow pyrites, that M. Haüy thought he ought to class them as two different mineral species. No difference in the composition of the two substances, however, can be discovered by chemical analysis; which adds one more to the exceptions to the general rule, furnished by the diversity in the two forms of carbonated lime, and that more recently observed by M. Mitscherlich in two forms of the surphosphate of soda.

White pyrites consists of two varieties; one of which, perfectly crystallized, stands the air; while the other, which presents a confused crystallization, effloresces on exposure to the air, and falls into a powder evidently vitriolic. This phænomenon proves, then, a difference of composition between the two varieties; a difference which deserves to be studied, in order that we may see whether it can explain the difference between them and yellow pyrites.

I allowed a piece of white pyrites to effloresce for two years and a half; and when it was entirely reduced I subjected it to examination. Its bulk was almost doubled; and it fell into pieces on the slightest touch. A part of the mass was converted into a white powder, of a styptic taste, and this powder had begun to grow yellowish towards the edges. Viewed through a microscope it presented a mass full of small cavities filled with a white and effloresced salt, the interstices between which appeared to be of white pyrites in an entire state, and more or less crystalline.

I then tried a portion of it with water; and separated the dissolved part from the indissoluble residue. The latter consisted partly of a rough powder, which was composed of small crystals of pyrites, and partly of a powder. finer and lighter and of a grayish colour approaching to black. Viewed through the microscope, this powder presented nothing but brilliant particles of pyrites without any trace of sulphur either separate or mingled with the pyrites.

(*a*.) The solution deposited on being brought into contact with the air, a yellow ochre; it was then entirely neutral.

I treated this solution with nitric acid in order to oxidate the excess of iron, and I afterwards decomposed it by means of muriate of barytes and caustic ammonia. The product was 2.03 gr. of sulphate of barytes; and after separating the ex-

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* From the *Annales de Chimie* for April 1822.

cess of muriate of barytes by sulphuric acid, 0.68 gr. of peroxide of iron, the proportions were exactly the same as the neutral sulphate of protoxide of iron (Fe S^2) would have given; for 29.16:9.78=2.03:6.809
The salt formed was equal to 0.74 gr. of proto-sulphuret of iron (Fe S^2); but the residue of the pyrites not decomposed, weighed 4.653 gr.; that is to say, six and seven times as much as the effloresced part.

(*b.*) To make sure that the undissolved residue did not contain any free sulphur, I dissolved some of the loosest of it by means of nitro-muriatic acid, until there was an entire acidification of the sulphur. There remained a little silex not dissolved. The solution furnished 0.64 gr. of oxide of iron and 3.82 gr. of sulphate of barytes, which perfectly accords with the composition of deuto-sulphuret of iron, that is to say, pyrites. Since therefore the effloresced part was a sulphate, with a protoxide for its base which did not contain any excess of acid, and since there were no traces of sulphur being separated during the efflorescence, it is evident that the effloresced part must have been a proto-sulphuret of iron, Fe S^2, which has not yet been found in an isolated state in the mineral kingdom*, and that the remainder, which was not liable to effloresce, had been a deuto-sulphuret, Fe S^4.

The efflorescent pyrites cannot therefore be any thing else than particles more or less crystallized of Fe S^4, cemented together by particles more or less numerous of Fe S^2, which convert themselves by little and little, by exposure to the air, into Fe S^2; the pyrites loses then its coherence in proportion as the cement of the crystallized particles is destroyed.

The efflorescence does not therefore furnish any clue to the solution of the question as to the difference between the forms of yellow and white pyrites.

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* It will be recollected that the magnetic pyrites which does not effloresce is a chemical combination of Fe S^4 + 6 Fe S^2.

XIII. *Notices respecting New Books.*

Sciography, or Examples of Shadows, and Rules for their Projection, intended for the Use of Architectural Draughtsmen; by Joseph Gwilt, Architect. 8vo. 9*s*.

Practical Hints on Composition in Painting; illustrated by Examples from the Great Masters of the Italian, Flemish, and Dutch Schools; by John Burnet. 4to. 12*s*.

An Encyclopædia of Gardening; comprising the Theory and Practice of Horticulture, Floriculture, Arboriculture, and

Landscape Gardening; by J. C. Loudon, F.L.S., &c. 8vo. 2*l.* 10*s.*

Supplement to Vol. IV. of the Transactions of the Horticultural Society of London. 6*s.*

The Rudiments of Perspective; in which the Representation of Objects is described by two Methods; by Peter Nicholson. 8vo. 14*s.*

A History of the British Empire, from the Accession of Charles I. to the Restoration; with an Introduction. By George Brodie, Esq. Advocate. 4 vols. 8vo. 2*l.* 12*s.* 6*d.*

The Philosophy of Zoology; or a General View of the Structure, Functions, and Classification of Animals. By J. Fleming, D.D., &c. &c. 2 vols. 8vo. 1*l.* 10*s.*

The Scottish Cryptogamic Flora; or Coloured Figures and Descriptions of Cryptogamic Plants found in Scotland, and belonging chiefly to the Order *Fungi.* By Robert Kaye Greville, Esq. F.R.S. E. &c. No. I. 4*s.*

XIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 6. On the Binomial Theorem, by John Walsh, Esq.

A paper, by Dr. Davy, was likewise read, entitled "Some Observations on Corrosive Sublimate."

June 13. On the State of Water and Aëriform Matter in the Cavities of certain Crystals, by Sir Humphry Davy, Bart. P.R.S.

June 20. Some Experiments on the Changes which take place in the fixed Principles of the Egg during Incubation. By W. Prout, M.D. F.R.S.

XV. *Intelligence and Miscellaneous Articles.*

EXPLOSION OF A STEAM-ENGINE BOILER.

Chester, July 11, 1822.

Sir,—On the 29th ult. a steam-engine boiler belonging to Mr. Boult, tobacco-manufacturer of this city, exploded with terrific violence; and as the effects of the explosion have been attended with the most afflicting circumstances, it is more than probable, that a relation of the accident in the Philosophical Magazine may elicit views in the mind of the scientific engineer, which may tend to lessen the amount of danger at present attendant upon the use of the steam-engine. I visited the spot a few hours after the explosion, and found a part of the premises completely destroyed and in ruins, the windows of the adjoining houses entirely broken, and a building nearly fifty yards from the scene of destruction set on fire by pieces of ignited fuel falling upon its roof.

The boiler employed in this manufactory was eight feet long, four feet broad, and five feet deep, and had recently undergone a thorough repair; it was connected with machinery requiring steam of great expansive force for its movements, and Saturday the 29th ult. was the first time of using it since the repairs.

The steam was speedily raised in such a very powerful manner, that, as I have since learned, the boiler was perceived to have an oscillating movement for a considerable time:—when the steam had attained this state, and while Mr. Boult with four of his men were anxiously looking on, the boiler suddenly burst, and in a moment spread desolation all around.

The situation of the persons present now became truly deplorable—buried in the falling ruins of the building, and scalded in a shocking manner: prompt assistance was however obtained, and every effort that surgical skill could command, was brought into action for the alleviation of the sufferings of the unfortunate individuals: but alas! four out of the five persons present on this occasion (amongst whom Mr. Boult himself) have fallen victims to the explosion.

Upon examining the boiler, I found the bottom completely torn away on one side from the seat of the vessel, and forced into the ash-pit; the plates of iron, half-inch in thickness, forming the bottom of the boiler, were twisted from the convex to the concave form, and the two surfaces of the severed plates presented comparatively smooth faces, with little or no appearance of raggedness.

Most unfortunately in the present case, neither the respected proprietor of the engine, nor indeed any of his men, had sufficient knowledge of the force of highly compressed steam, to render the working of the engine safe in their hands. In the instance before us no doubt is entertained that the safety valve was overloaded; the boiler, on ordinary occasions, worked at a pressure of thirty pounds to the square inch; but with boldness the most fatal, it was on this occasion exposed (there is reason to believe) to a pressure of upwards of one hundred pounds. And here let us pause for a moment to inquire whether the present mode of constructing steam valves be entitled to unlimited confidence? In my opinion, no valve ought to be deemed safe unless made perfectly independent of the most careful workman that ever existed. The very possibility of a safety valve being by *any means* 'over-weighted implies a defection of principle: and when the almost universal employment of steam as a mechanical agent in Great Britain is considered, it cannot but excite every man's surprise, that so important a principle as *absolute safety* should have been so long unheeded by our engineers[*].

I am, sir,
Your most obedient and humble servant,
To Dr. Tilloch. S. Leet.

OIL GAS.

The Quarterly Journal of Science announces that Mr. Wilson proposes obtaining this gas, by introducing the seeds which contain oil into the retorts. The editors very properly add, however, that they greatly doubt the plausibility of this proposal; and it appears to us, to be an example of retrograding in physical science.

One of the great merits of Taylor and Martineau's apparatus for producing oil gas, as now perfected by them, is its small bulk compared with its power of production, and the facility with which the oil may be supplied to the retort in graduated quantities, and so of maintaining or stopping the supply of gas at pleasure. By this, also, decomposition is effected by the smallest possible quantity of fuel; for the retort being once made slightly red hot is easily kept so, while the oil enters it only drop by drop, and being completely brought into contact with the heated matter, the operation is as complete as possible.

Now, instead of this, it is proposed to adopt a plan which requires that the whole charge of a very bulky material should be put into the retort at once, and wherein the oil, which is the only part that can produce a good gas, is so enveloped by matter which is a bad conductor of heat, that one can hardly imagine how it is to be efficiently decomposed, unless such a fire be employed as will require large supplies of fuel, and occasion a rapid destruction of the retorts.

SPECIFIC GRAVITY.

Mr. Creighton has communicated to the Editor of the Quarterly Journal of Science, a notice of a very simple and ingenious instrument for determining the specific gravities of solid bodies.

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[*] Our correspondent, Mr. Leet, is not fully informed on the subject of the safety valve. This is no longer a desideratum in mechanical science; but the controul of the engine must belong to some one: and if both master and servant be ignorant of its nature, no steam valve, however constructed, can give them knowledge.—Edit.

It consists of two cages of wire, which are suspended the one under the other to a sensitive spiral spring. The lower cage being immersed in water, the weight of the body in air will first be indicated by the tension of the spring when it is placed in the upper cage; by then removing it to the lower one its weight in water will be pointed out on the graduated scale: and Mr. Creighton gives a formula for ascertaining the specific gravity from these two observations, without recurring to the usual tedious calculations.

ARSENIURETTED HYDROGEN GAS.

The following method of preparing this gas is proposed by M. Serrulas: Two parts of bitartrate of potash, two parts of antimony, and one part of arsenious acid are to be mixed and well triturated in a mortar, and then strongly heated for two hours in a close crucible. The alloy thus produced, when in contact with water produces hydrogen gas saturated with arsenic, which may be preserved for any length of time in close vessels. The gas is procured by putting, quickly, about 150 grains of the alloy, previously reduced to coarse powder, under a jar filled with water, and inverted in a glass basin containing water.—*Journal de Physique.*

ANALYSIS OF BLACK HELLEBORE.

A late analysis of the roots of black hellebore by MM. Feneulle and Capron yielded,—1. a volatile oil; 2. a fatty matter; 3. a resinous matter; 4. wax; 5. a volatile acid; 6. a bitter principle; 7. mucus; 8. alumina; 9. gallate of potash and acidulous gallate of lime; 10. a salt with an ammoniacal base.—*Journal de Phar.*

ANALYSIS OF THE ROSE.

From 1000 grammes of the petals of the white Provençal rose lately analysed by M. F. Cartier, he obtained by incineration a residuum of 99 gr., containing subcarbonate of potash, phosphate, and traces of muriate of potash, carbonate of lime, phosphate of lime, traces of phosphate of magnesia, silica, and oxide of iron. The quantity of the last was ascertained to be 12·5. From 1000 gr. of red roses he procured only 50 of residuum, containing no more than 4 oxide of iron.—*Journal de Phar.*

EFFECT OF HEAT AND COOLING ON THE COLOUR OF THE RUBY.

Dr. Brewster relates the following singular changes in the colour of rubies while cooling after exposure to a high degree of heat. At a high temperature the red ruby becomes green:

as the cooling advances it becomes brown, and the redness of this brown gradually increases till the ruby recovers its primitive brilliant red colour. A green ruby suffered no change from heat. A blueish green sapphire became much paler at a high heat, but resumed its original colour by cooling.—*Edin. Phil. Journal.*

DIOCHRISM OF TOURMALINE.

A specimen of diochritic tourmaline in the cabinet of Mr. Allan exhibits the following singular contrast of colours. The plate is cut perpendicular to the axis of double refraction, and also to the axis of the prism. In the direction of the axis the colour is a deep brilliant blue, and in the direction at right angles to the axis the colour is a very pale red, approaching to pink.—*Edin. Phil. Journal.*

DAMP IN WALLS.

An easy and efficacious way of preventing the effects of damp walls upon paper in rooms, has lately been used (and as we understand) with complete success. It consists of lining the wall or the damp part of it with sheet lead, purposely rolled very thin: this is fastened up with small copper nails, which not being subject to rust are very durable, and the whole may be immediately covered with paper.

The lead is not thicker than that which is used in the chests in which tea is imported, and is made in sheets of which the width is about that of common paper-hangings. We have seen some which was rolled at the lead-mill of Messrs. Hutchinson and Co. at Pateley Bridge, in Yorkshire, and which was as light as eight ounces and even four ounces to a square foot, and yet quite impermeable to water.

The remedy for a very disagreeable occurrence is thus rendered not only easy but very cheap.

STUPENDOUS CAVERN.

There was discovered about three weeks since, on the north bank of the Black River, upon the land of James Le Ray, esq. opposite to the village of Watertown, an extraordinary cavern, or grotto, the mouth of which is about ten rods from the river, north of the Falls and of Cowan's Island.

The great extent of the cavern, and the great number of spacious rooms, halls, and chambers, into which it is divided, and the immense quantities of calcareous concretions which it contains, and different states of those concretions, from the consistence of lime mortar to that of the most beautiful stalactites as hard as marble, render it difficult, if not impossible, to describe it; and I shall only attempt to give a faint description of three or four rooms.

The mouth of the cavern is in a small hollow, about five feet below the surrounding surface of the earth—you then descend sixteen feet and a half into a room about sixteen by twenty feet, and eight feet high, and behold in front of you a large flat or table rock, twelve or fourteen feet square, two feet thick, and elevated about four feet from the bottom of the cavern, the roof over-head covered with stalactites, some of which reach to the table rock. On your left hand is an arched way of one hundred and fifty feet, and on your right hand is another arched way, six feet broad at the bottom, and six feet high, which leads into a large room. Passing by this arch about twenty feet, you arrive at another, which leads into a hall ten feet wide and 100 feet long, from five to eight feet high, supported with pillars and arches, and the sides bordered with curtains, plaited in variegated forms, as white as snow. Near the middle of this hall is an arched way, through which you pass into a large room, which, like the hall, is bordered with curtains, and hung over with stalactites. Returning into the hall, you pass through another arch into a number of rooms on the left hand, curtained, and with stalactites hanging from the roof. You then descend about ten feet, into a chamber about twenty feet square and two feet high, curtained in like manner, and hung over with stalactites. In one corner of this chamber, a small mound is formed about twelve feet in diameter, rising three feet from the floor; the top of which is hollow and full of water from the drippings of stalactites above, some of which reach near to the basin.

Descending from this chamber, and passing through another arch into a hall, by the side of which you see another basin of water, rising about four inches from the floor; formed in the same way, but the shape, size, and thickness, of a large tea-tray, full of the most pure and transparent water.

The number and spaciousness of the rooms, curtained and plaited with large plaits, extending along the walls from two to three feet from the roof, of the most perfect whiteness, resembling the most beautiful tapestry, with which the rooms are embroidered; and the large drops of water, which are constantly suspended on the points of innumerable stalactites, which hang from the roofs above; and the columns of spar resting on pedestals, which, in some places, appear to be formed to support the arches above—the reflection of the lights, and the great extent and variety of the scenery of this amazing cavern,—form altogether, one of the most pleasing and interesting scenes that was ever beheld by the eye of mortal man.

Its discovery immediately drew to it great numbers of people from the village and surrounding country, who were making great depredations upon it, by breaking off and carrying away whatever they esteemed most curious; when Samuel C. Kennedy, esq. Mr. Le Ray's agent, was applied to, to prevent further spoliation; who immediately directed the passage into the cavern to be enlarged, stairways made, with a strong door placed under a lock and key, which has been finished, and the door closed.

The discovery of this grotto, added to the extensive petrifaction along the river in this vicinity, especially on Cowan's Island, of the once inhabitants of the deep, cannot fail to render Watertown (to the curious at least) a lasting place of resort.

It may be proper to mention here that the cavern has been but partially explored, and that no one who has been into it, although some suppose they have travelled more than 100 rods, pretend to have found the extent of it, or to know the number of rooms, halls, and chambers which it contains.—*The (American) Watertown Republican of May* 14, 1822.

SINGULAR PHÆNOMENON.

A number of hagberry trees, growing on the banks of the Girvan, about a mile beyond Kirkmichael, are at present entirely divested of their foliage, and covered with a sort of silky substance resembling in texture and appearance the finest cambric paper, but much stronger, which is occasioned by myriads of small worms. These reptiles are first seen in an inactive state hanging in large clusters under the branches. On bursting from embryo they commence to crawl up and down the trunk and branches, each emitting a small slimy thread somewhat finer than the spider's, which, from their incalculable numbers, unite together, and form this singular substance, which covers the trees, and imparts to them, when viewed from a distance, the appearance of blighted trunks covered with snow. Some hundreds of these insects are at times observed suspended by as many threads, which are spun out till they join another branch, and form a passage across.

EXPEDITION TO THE ROCKY MOUNTAINS.

A company of 180 adventurers are stated to have left St. Charles, Missouri, on the 10th of April, for the Rocky Mountains. They are described to be of vigorous and masculine appearance, well armed, and prepared for a three years tour through this almost unknown and savage country. This expedition, it is added, can be truly said to be composed of the yeomanry of Missouri, who have embarked in an enterprise, which, if they are successful, will not only be very profitable to themselves, but a great national benefit, in laying the foundation for an extensive fur trade, and proving to the effeminate sons who remain at home, that activity is the true source of wealth and greatness. It is their intention to pass over to the Colombia, and from thence to the ocean. Trapping and hunting furs is their principal object, which the experience of those who have heretofore engaged in this business on a small scale has proved to be lucrative.—*American Paper.*

AID IN CASE OF SHIPWRECK.

Several experiments have recently been made before the Trinity Board, and a Board of General Officers, at Woolwich, on a new plan for affording speedy and effectual aid in case of shipwreck. It differs from Captain Manby's plan, inasmuch as the line of communication can be made by means of a rocket instead of a mortar. A roller is also added, and so admirably constructed, as to render considerable facility and safety in reaching the shore. The advantage that must be derived in the night time, from the rocket, is obvious, as it is so constructed that it will burn in the water. The precision, by which the line of communication is formed, is also considerably augmented, and the safety of life and property consequent upon having the apparatus ready on board, in case of accident, is paramountly enhanced. The two Boards have spoken in appropriate terms of the new plan, and have made their report accordingly.

HUSBANDRY.

That variety of *Phalaris arundinacea*, which is frequently planted in our gardens, and is called *ribbon grass*, or *striped grass*, might be cultivated to very considerable advantage by the farmer. It is a very hardy plant; it affords excellent food for cattle; it may be cut three or four times in the summer: and what is not the least of its merits, it will produce an earlier crop than almost any other grass. It thrives very well on dry ground, though it rather prefers moist situations. It has been tried in the South of England with success. A quarter of an acre of land dedicated to experiments on this grass will not be wasted. It is very easily cultivated, as it grows rapidly from bits of the roots being planted in the ground, either by ploughing them in with a very shallow furrow, or by setting them in a hole made by a pointed stick.

COPPER RAISED IN GREAT BRITAIN.

In Cornwall during the last six months ending with June, there have been 67 copper mines which have brought ores to sale at the public ticketings, the gross produce of which has been as under,

Copper ore	52125 tons.
Fine copper	4433 tons.
Value of the ore	338,845*l.* 15*s.* 6*d.*

Average produce of the ore in metal 8½ per cent.

Price of copper or standard 108*l.* 15*s.* 0*d.* per ton.

Of this nearly one half is produced by six principal mines, as follow:

	Ore. Tons.	Copper. Tons.	Value of Ore. £.	*s.*	*d.*
Consolidated mines	6772	575	44,270	12	6
Dolcoath	5146	352	27,127	1	6
United Mines	3342	313	23,813	14	0
Wheal Abraham, &c.	4417	302	22,208	4	6
Pembroke	3215	251	18,395	6	6
Tuskuby	2371	249	19,572	17	6
	25263	2042	£155,387	16	6

The aggregate quantity of fine copper produced by all the mines of Great Britain and Ireland, will appear from the following statement, which is made up for one year ending 30th June 1822.

	Copper Ore. Tons.	Fine Copper. Tons.	Value of Ore. £.	*s.*	*d.*
Sold at the ticketings in Cornwall	104,522	9140	663,085	13	2
Sold at Swansea from Ireland and various parts of England and Wales	3,539	388	30,967	12	6
Devon		532	about 46,000	0	0
Sundry private sales		184	do. 16,000	0	0
Anglesea (estimated at)		600	do. 45,000	0	0
		Tons 10844	£801,053	5	8

CURIOUS MASS OF IRON AND ZINC.

A curious concrete mass of iron and zinc, in weight more than a pound, has been presented to the Liverpool Royal Institution from a friend in London. It is part of the *residuum* which remained in an oven in which some millions of bank notes had been burnt, and is supposed to have been amalgamated from the materials which have entered into the composition of the ink.

THE SPINAL MARROW.

Dr. Tiedemann (of America) endeavours to demonstrate, that instead of the spinal marrow being a continuation or prolongation of the brain, it is the latter and the cerebellum to boot, which proceed from the spinal marrow; alias, that the brain and cerebellum are an *efflorescence* of the marrow in question.

AMERICAN ELKS.

A description of the animal was read not long since at the Linnean Society. It is also figured in Fred. Cuvier's work now publishing in numbers. There is also a figure and some account of it in an early number of the Colonial Journal.

A pair of the beautiful and gigantic non-descript Elks, known by the Indians of the Upper Missouri (where they have been lately discovered) by the name of Wappeti, arrived at Liverpool on Tuesday the 9th of July, on their way to London. These noble animals are the size of the horse with immense spreading horns; their form the most perfect model of strength and beauty, combining the muscular strength of the race-horse, with the lightness and agility of the greyhound; are capable with ease of drawing a carriage or carrying a person more than 20 miles an hour. They are perfectly domesticated, and of the most amiable and gentle disposition.—*Liverpool Advertiser.*

A NEW INSTRUMENT FOR CATARACT.

Professor Gibson, of the University of Pennsylvania, has lately invented an instrument exceedingly well calculated for cutting to pieces the crystalline lens, in all cases of cataract. It is well known that the knives of Saunders, Sir William Adams, and other oculists, are defective, in not being sufficiently strong and sharp to divide the lens when it happens to be of a very hard texture. Under such circumstances, the cataract is soon dislocated, in the attempt to divide it by the knife; and is either depressed into the vitreous humour, or else enucleated from its capsule. To obviate these inconveniences, Dr. Gibson has contrived a scissor so delicate, as hardly to exceed in size the iris knife of Sir William Adams, and, at the same time, so strong and sharp, as to cut, with ease, the most solid and compact lens and capsule, without injuring in the slightest degree any part of the eye. These scissors are formed upon the principle of Dr. Wollaston's scissors, used for common purposes—with the edge so constructed as to operate like a knife. On this account, the instrument perforates the coats of the eye with the utmost facility; and when introduced, the blades can be opened to a certain extent, so as to cut the lens to pieces, without bruising it or any other part—the necessary effect of scissors, as they are usually made. This instrument possesses another advantage. The lens is supported in its natural situation during the operation, by having one blade behind, and the other before it; so that it may be cut to pieces, *in situ;* and it remains afterwards forced by the shut blades, into the anterior chamber, for dissolution. In this way the operations of Saunders and Adams may be performed with great effect, and without that necessity for repetition which so often occurs when the common instruments are employed. Dr. Gibson has made trials with the instrument, sufficient to convince him of its decided superiority over every other used for the same purpose.

GASEOUS SPRING.

About a quarter of a mile below the village of Milan, in the state of Ohio, is a place just in the edge of the water of the Huron river, where there is a constant current bubbling from a number of places. These bubbles when touched with a lighted candle or torch, burn with a beautiful clear and brilliant blaze. There is gas enough issues to light the houses.—*American Paper.*

IMPROVED HORSE-SHOES.

Col. Goldfinch, of Hythe, has obtained a patent for a new method in the formation of horse-shoes. The improvement consists in making the horse-shoe in two parts, or separating it in two pieces, by cutting it through near the toe. The object of the contrivance is, that the frogs of the horse's hoof may be enabled to expand and grow in a healthy state. The separation is to be made in an indented form, and the two parts fastened together by pins. It is further proposed to attach the shoe to a horse's hoof by driving the nails obliquely, as in the French manner of shoeing. For this purpose, the situations of the nail-holes are to be from about one third to half the width of the shoe distant from its outer edge, and tending in a slanting direction outwards.

MATHEMATICAL DISCOVERY.

Mr. Herapath, of Cranford, who has lately become known by his physico-mathematical writings on the cause and laws of heat, attraction, and other phænomena, has just finished a discovery of the greatest importance in the mathematics. It is well known that Newton and succeeding mathematicians have extended their inquiries merely to fluxions and fluents or differentials and integrals of the form $d^{\pm 1}$, $d^{\pm 2}$, $d^{\pm 3}$, $d^{\pm 4}$, &c. with indices in whole numbers only; and to functions of the same form $\psi^{\pm 1}$, $\psi^{\pm 2}$, &c. regarding others, with even simple rational fractions, as transcending the powers of analysis. Mr. Herapath has however discovered a method of great simplicity and beauty, which is applicable to differentials, integrals, and functions of every possible form; whether the indices be whole, fractional, rational, irrational, or even functional, and imaginary. It has besides the advantage of not being confined to any particular species of calculation; but is investigated on those general principles, that it extends itself with equal facility to every kind of calculus that is or appears likely to be discovered; and if the direct calculus be possible, it always makes the inverse or any real function of it equally possible.

THE COMET OF 1822. BY M. NICOLLET*.

The comet discovered at Marseilles on the 12th of May last, by M. Gambart, was observed at Paris for the first time on the 18th of the same month. Since that time the astronomers of the Royal Observatory have never ceased to watch its course. The fine weather having permitted us to make a great number of observations, I have been able to calculate and to deduce the parabolic orbit as follows:

The course by the perihelium 6th May 1822, at 3 hours 5 minutes and 11 seconds in the morning.

Distance of the perihelium	0.504220
Inclination of the orbit	53° 34′ 3″
Longitude of the ascending node ...	177° 30′ 50″
Longitude of the perihelium in the orbit	192° 48′ 45″

The heliocentric movement—retrograde.

This comet does not resemble in its elements either the comet of 1204, or any of the comets that have been observed up to the present time. It is very small, and has very little appearance of a tail. Its distance from the earth increases every day. On the 18th of May this distance was nearly equal to that of the sun; but on the 31st of that month it was one-half of the distance further from the earth. The comet is not visible to the naked eye; but the astronomers have been enabled by the favourable state of the weather to continue the observations, and to furnish also the means of perfecting the preceding elements.

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* *Annales de Chimie* for May 1822.

EARTHQUAKE.

On the morning of the 10th of July, at a quarter before seven, there was a violent shock of an earthquake at Lisbon, which lasted six or seven seconds. The oscillation was rather perpendicular than horizontal. It consisted of two smart shocks within a short interval. The shocks seem to have been more severe on the other side of the Tagus, and the buildings near the sea were most affected; but we have not learnt any particulars of its effects.

LIST OF PATENTS FOR NEW INVENTIONS.

To Marc Isambard Brunel, of Chelsea, Middlesex, engineer, for certain improvements on steam-engines.—Dated the 26th June 1822.—6 months allowed to enrol specifications.

To Thomas Gauntlett, of Bath, Somersetshire, surgeons' instrument-maker, for certain improvements on vapour-baths, by which the heat is better regulated and the baths rendered more portable.—26th June.—2 months.

To William Brunton, of Birmingham, Warwickshire, engineer, for certain improvements upon fire-grates, and the means of introducing coal thereon.—26th June.—6 months.

To Louis Barnard Rabaut, of Skinner-street, Snow-hill, London, gentleman, for an improved apparatus for the preparation of coffee or tea.—26th June.—6 months.

To Thomas Postans, of Charles-street, in the parish of St. James, gentleman, and William Jeakes, of Great Russell-street, Bloomsbury, ironmonger, for an improvement on cooking apparatus.—26th June.—2 months.

To George Smart, of Pedlar's Acre, Lambeth, Surrey, civil engineer, for his improvement in the manufacture of chains which he denominates 'mathematical chains.'—4th July.—6 months.

To Joseph Smith, of Sheffield, Yorkshire, book-keeper, for an improvement of or in the steam-engine boiler.—4th July.—6 months.

To John Bold, of West-street, Bermondsey, printer, for certain improvements in printing.—4th July.—6 months.

METEORO-

METEOROLOGICAL TABLE,

By Mr. Cary, of the Strand.

Days of Month. 1822.	Thermometer.			Height of the Barom. Inches.	Weather.
	8 o'clock Morning.	Noon.	11 o'clock Night.		
June 27	65	71	63	30·20	Fair
28	61	68	58	·08	Cloudy
29	56	58	57	·10	Showery
30	56	62	60	·01	Cloudy
July 1	60	68	62	·14	Fair
2	60	67	61	·01	Cloudy
3	57	72	62	·03	Fair
4	63	76	68	·04	Fair
[illegible]	67	67	60	29·80	Thunder-shower
6	60	67	58	30·02	Cloudy
7	58	71	56	·15	Fair
8	57	68	60	·25	Fair
9	60	69	59	·10	Showery
10	60	72	63	29·92	Showery
11	63	67	58	·83	Showery
12	59	67	56	·43	Showery
13	59	67	55	·99	Fair
14	55	70	59	30·07	Fair
15	59	69	58	29·97	Showery
16	58	67	59	·77	Showery
17	60	69	59	·76	Fair
18	63	74	60	·80	Showery
19	62	72	61	·57	Fair
20	60	70	60	·61	Showery
21	61	70	63	·63	Cloudy
22	62	70	62	·82	Rain
23	64	69	62	·81	Rain
24	64	70	60	·68	Fair
25	61	70	59	·75	Showery
26	60	68	58	·83	Showery

N.B. The Barometer's height is taken at one o'clock.

THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL.

31st AUGUST 1822.

XVI. *On the Hygrometer by Evaporation.* By J. Ivory, *M.A. F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

1. The idea of employing the evaporating power of the atmosphere for discovering its condition in regard to moisture, is due to the late most ingenious philosopher Dr. James Hutton of Edinburgh. He dipped a thermometer in water, both being previously brought to the general temperature; and then exposing the wet bulb to a current of air, he marked how many degrees the mercury sunk in the tube; and he estimated the dryness of the air by the quantity of the depression. This is certainly a very curious experiment; and if we go back to the time at which it was made, before 1792, it will perhaps be admitted that no accurate and complete account of it could then be given, for want of a sufficient knowledge of the laws that obtain between permanent elastic fluids and the vapours mixed with them. The experiment is deserving of discussion chiefly in two respects:—Is the degree of the depression of the thermometer dependent only on the quantity of moisture in the atmosphere, without being affected by accidental circumstances? What is the relation between the depression of the thermometer and the tension of the aqueous vapour diffused in the atmosphere?

Professor Leslie, of Edinburgh, has determined that the cold produced in Dr. Hutton's experiment is owing entirely to the drying quality of the atmosphere, and that it is not influenced by any other circumstances; and applying that very delicate instrument, his differential thermometer, to measure the degree of depression, he has constructed the hygrometer that bears his name. This instrument has been many years before the public, and it has been too ably described by its learned inventor, in many different publications, to require any further development of its principles or properties. On the present occasion our intention reaches no further than to discuss the original experiment made by Dr. Hutton.

2. We may suppose two different thermometers; one for marking the general temperature; and the other, having its bulb dipped in water, or covered with a wet linen rag, or with bibulous paper soaked in water, for the purpose of ascertaining the cold produced by the evaporating power of the air. Now, if we suppose that the atmosphere is saturated with moisture, its contact with the wet bulb will produce neither any evaporation from the humid surface, nor any deposition of moisture upon it from the air. As there is no action between the air and the bulb, neither heat nor cold will be produced; and the mercury will stand at the same height in both thermometers. But if the air be not saturated with moisture, there will be an evaporation from the wet bulb; and, evaporation being invariably attended with the disappearance of heat of temperature which enters into the constitution of the vapour in a latent form, the mercury of the thermometer will be depressed. The air in contact with the moist surface will therefore become damper both by the depression of its temperature and by the small addition of humidity diffused in it. If, in a single instant of time, the point of saturation with humidity be not fully attained, the same process will be repeated in a second and a third instant, until at length the air is charged with all the vapour it is capable of retaining at its depressed temperature.

It is certain that the thermometer cannot be depressed below the point at which the air is saturated with humidity. Even if, by any accidental cause, we suppose a greater degree of cold to be produced, the consequence would be a deposition of part of the vapour in the liquid form, attended with an evolution of heat; by which means, supposing the extraneous cooling cause to be removed, the thermometer would again rise and settle at the point of saturation.

In perfectly still air it is known that evaporation goes on very slowly; and this circumstance may make it difficult to discern when the action between the air and the bulb has come to an invariable state. Some agitation of the instrument, or some motion in the air, seems to be proper, whether to hasten the permanent degree of cold, or to ascertain that it has actually taken place.

On the other hand, every portion of air must take up some definite time in parting with its heat and absorbing its share of vapour; and if we suppose a circulation so brisk as to displace the air from the thermometer in half that time, it is plain that it would be only half saturated with moisture by its contact with the wet bulb; in which case the experiment would be unsuccessful. This however is only an extreme case, which it would be easy to guard against, were the effect well ascertained by experiment.

Another disturbing cause it seems difficult entirely to obviate. For the stem of the thermometer, having constantly the general temperature of the atmosphere, will communicate some of its heat to the colder bulb; and although the air which touches the bulb may in this case be fully saturated with humidity, yet the heat extricated from it in cooling will be less than the latent heat of the vapour diffused in it: whereas the success of the experiment requires an exact equality between these two quantities of heat.

In this experiment it is presumed that all the air that comes in contact with the wet bulb is continually saturated with vapour, and likewise that all the heat necessary for feeding the evaporation is furnished by the same air. If the air be displaced from the humid surface without being saturated with moisture; or if the heat that enters in a latent form into the constitution of the rising vapour, be derived from any other source than the air that comes successively into contact with the wet bulb; in both these cases the experiment will fail as a means of discovering the state of the atmosphere in regard to moisture. The effects of the disturbing causes that have been enumerated, do not appear to be so formidable as to endanger the success of the experiment to a degree of accuracy sufficient for most practical purposes.

3. If we now suppose that the temperature of the thermometer is stationary, and that every portion of air becomes saturated with moisture by its contact with the wet bulb, and likewise that it deposits just as much of its heat as is contained in a latent form in the vapour it absorbs; we are led to this question:

Prob. A given quantity of air, as a cubic foot, under a given pressure, having a given temperature, and containing vapour of some unknown tension, being supposed to have its temperature depressed by any cause, and likewise to have the vapour due to all the heat extricated in cooling continually diffused in it, till at length it becomes saturated with moisture at some known temperature; it is required to find the unknown tension of the aqueous vapour contained in the air.

By the vapour due to a certain quantity of heat is meant, the vapour which contains the proposed heat in a latent form; and it is supposed that the same quantity of heat is absorbed in the formation of vapour at all temperatures.

To solve this problem, let b denote the height of the barometer, τ the temperature estimated on the centigrade scale,

and

and x the unknown tension of the aqueous vapour: then the weight of a cubic foot of the atmospheric air will, in these circumstances, be equal to the weight of a cubic foot of dry air under the pressure $b-x$, and the weight of a cubic of vapour of the tension x, the common temperature being τ.

Let w stand for the weight of a cubic foot of vapour at the temperature zero, the tension, as determined by Dalton, being $\frac{1}{5}$ of an inch of the barometric column: then the weight of a cubic foot of dry air at the same temperature and pressure will be $\frac{8}{5} \times w$*; and consequently the weight of a cubic foot of the same air at the same temperature and under the pressure 30 inches, will be $5 \times 30 \times \frac{8}{5} \times w$, or $240 \times w$.

Now, $240 \times w$ being the weight of a cubic foot of dry air at the temperature zero and pressure 30 inches, the weight of the same volume when the temperature is τ, and the pressure $b-x$, will be

$$240 \times w \times \frac{b-x}{30} \times \frac{1}{1+m\tau},$$

where $m = \cdot 00375$, or the expansion for one centesimal degree: and the weight of a cubic foot of vapour of the temperature τ, and tension x, will be $5w \times \frac{x}{1+m\tau}$. Wherefore the weight of a cubic foot of atmospheric air originally considered will be thus expressed, viz.

$$240w \times \frac{b-x}{30} \times \frac{1}{1+m\tau} + 5w \times \frac{x}{1+m\tau}.$$

Let a stand for the specific heat of air, and s for that of vapour: then we get

$$\left\{ 240w \times \frac{a}{1+m\tau} \times \frac{b-x}{30} + 5w \times \frac{x.s}{1+m\tau} \right\} \times \delta\tau$$

for the weight of water that would be raised one degree of the thermometer, by the heat extricated in cooling the mixed mass of air and vapour a number of degrees expressed by $\delta\tau$.

Again, put λ for the latent heat of steam: then

$$\left\{ 240 \times \frac{a}{1+m\tau} \times \frac{b-x}{30} + 5w \times \frac{x \cdot s}{1+m\tau} \right\} \times \frac{\delta\tau}{\lambda}$$

will be the weight of steam due to the above-mentioned quantity of heat; or the weight of vapour that contains in a latent state all the heat extricated in cooling the mass of air to the temperature $\tau - \delta\tau$.

Wherefore we have the following weight of dry air and vapour

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* Biot's *Traité de Physique*, vol. i. pp. 296, 297.

pour at the temperature $\tau-\delta\tau$, instead of the original cubic foot of mixed air, viz.

$$240 \times \frac{1}{1+m\tau} \times \frac{b-x}{30} + 5w \times \frac{x}{1+m\tau}. \qquad \text{(A)}$$

$$+ \left\{ 240\,w \times \frac{a}{1+m\tau} \times \frac{b-x}{30} + 5w \times \frac{x \cdot s}{1+m\tau} \right\} \times \frac{\delta\tau}{\lambda},$$

the first term only being air, and the rest vapour.

The air at the reduced temperature $\tau-\delta\tau$, and with its new accession of humidity, is in a state of saturation. Its tension ϱ will therefore depend only on the temperature $\tau-\delta\tau$; insomuch that when one of these quantities is given, the other may be thence found by means of the Tables and formulæ usually given in this part of natural philosophy. Now at the temperature $\tau-\delta\tau$, and under the pressure b, the weight of a cubic foot of air mixed with vapour at the tension ϱ, will be equal to,

$$240\,w \times \frac{1}{1+m(\tau-\delta\tau)} \times \frac{b-\varrho}{30} + 5w \times \frac{\varrho}{1+m(\tau-\delta\tau)}, \qquad \text{(B)}$$

the first term being air, and the second vapour.

But, in any two parcels of the same air, there must be the same proportion between the two constituent parts. I therefore divide the air in the expression (A) by the air in the expression (B); and likewise the vapour of the former expression by the vapour of the latter: and as the two quotients must be equal, I get this equation, viz.

$$\frac{b-x}{b-\varrho} = \frac{5x + \left\{ 240\,a \times \frac{b-x}{30} + 5xs \right\} . \frac{\delta\tau}{\lambda}}{5\varrho}:$$

which, by the usual reduction, becomes

$$x . \left\{ 1 + \left(s - \frac{8a}{5}\right) . \left(1 - \frac{\varrho}{b}\right) . \frac{\delta\tau}{\lambda} \right\} = \varrho \left\{ 1 + \frac{8a}{5} . \frac{\delta\tau}{\lambda} \right\} - \frac{b}{30} . \frac{48a.\delta\tau}{\lambda}.$$

Now in the 4th vol. p. 726, of Biot's *Traité de Physique*, I find $a = {\cdot}2669 = \frac{4}{15}$ nearly, $s = {\cdot}847 = \frac{17}{20}$ nearly, and, at p. 713, $\lambda = 550$: and, by substituting these numbers, it will be found that the coefficient of x is very nearly equal to that of ϱ: the same coefficients also are both little different from unit, because $\delta\tau$ is always only a small part of λ: and, by means of these considerations, the foregoing equation may be simplified without detracting almost in any degree from its exactness, viz.

$$x = \varrho - \frac{b}{30} \times \frac{48\,a.\delta\tau}{\lambda}. \qquad (1)$$

Further, because $\frac{b}{30}$ is in most cases very nearly equal to unit, we get

$$x = \varrho - \frac{48a.\delta\tau}{\lambda}; \qquad (2)$$

that is, in words, "The actual tension of the vapour in the atmosphere is less than the maximum tension at the reduced temperature $\tau-\delta\tau$, by a quantity proportional to the observed depression."

If we were in possession of any different method of finding the tension of the vapour in the atmosphere, we should be able both to correct the coefficient of $\delta\tau$ in the foregoing formula, which number is liable to some uncertainty, and likewise to judge of the degree of accuracy of which Dr. Hutton's experiment is susceptible. Now such a method of experimenting has been employed by Dalton. It consists in exposing to the air a clean and dry surface of glass cooled artificially till it is observed that moisture begins to be deposited on it. When this takes place, the air in contact with the cold surface is in a state of saturation; and, the temperature of the *dewing point* being thus determined, it is easy to deduce from it the tension of the vapour at the actual temperature of the atmosphere.

Let y denote the weight of vapour contained in a cubic foot of the air at the general temperature τ; and h, the weight of vapour in a cubic foot at the reduced temperature $\tau-\delta\tau$; then, according to what has already been shown, we have

$$y = \frac{5w}{1+m\tau} \times x,$$

$$h = \frac{5w}{1+m(\tau-\delta\tau)} \times \varrho;$$

And hence,

[illegible] $$= \frac{y}{5w} \times (1+m\tau)$$

$$\varrho = \frac{h}{5w} \times (1+m\tau) \times (1-m\delta\tau);$$

and, by substituting these values in the formula (2), we get

$$y = h\,(1-m\delta\tau) - \frac{240\,a\,w}{\lambda} \times \frac{\delta\tau}{1+m\tau};$$

and, by omitting insensible quantities,

$$y = h - \frac{240\,a\,w}{\lambda} \times \delta\tau, \qquad (3)$$

or, in words, "The quantity of vapour in a cubic foot of air at the actual temperature τ, is less than the maximum of humidity which the air is capable of retaining at the reduced temperature $\tau-\delta\tau$, by a quantity proportional to the observed depression."

If we denote by H the maximum of humidity in a cubic foot of the air at the temperature τ; then

$$H = h + \frac{240\,a\,w}{\lambda} \times \delta\tau, \qquad (4)$$

will express how much the vapour really contained in the air falls short of saturation; and this seems to be the proper hygrometric quantity, or the just measure of the drying quality of the atmosphere.

It is proper to observe that the formula for computing H when τ is given, is

$$\text{Log. } H = A\tau + B\tau^2 + \&c.;$$

whence it follows that $H - h$ is not proportional to $\delta\tau$, but to $H \times \delta\tau$. The measure of the dryness of the air is therefore a quantity which varies both with the observed depression and the actual humidity of the atmosphere.

It appears therefore that the experiment of Dr. Hutton will fully disclose the condition of the atmosphere with regard to moisture. Nothing more is necessary for this purpose than to provide a table containing, in one column, the maximum tensions of the vapour for all degrees of temperature, such as is found in vol. 1. of Biot's *Traité de Physique*, and, in another column, the weights of vapour in a given volume, that correspond to the different tensions; quantities which, for every tension ϱ and temperature τ, are computed by the formula, $5w \times \frac{\varrho}{1+m\tau}$.

4. There is a great analogy between the experiment of Dr. Hutton and the method employed by Dalton for finding the Dewing point. In the latter method the air is brought to a state of saturation by the agency of cold alone; in the former the same effect is produced by the joint operation of cold and the addition of humidity. But Dalton's process cannot be used without some complication of machinery and the employing of artificial means for producing cold; whereas Dr. Hutton's experiment requires no more than the exposing of the wet bulb of a thermometer to the action of the atmosphere. In the one case, great nicety of observation is required for detecting the precise moment when the dew begins to be deposited; and from this cause considerable uncertainty, it is natural to think, must often arise, more especially at low temperatures, when the air contains only a very small portion of humidity. In the other case, the observer has only to read off the degree on the scale of a thermometer; and it appears that any want of precision in the experiment will cause only a proportional inaccuracy in the result; insomuch that the quantity of moisture in the atmosphere may be known with an exactness sufficient for most practical purposes, even if we suppose an uncertainty of half a degree, or a whole degree, in the quantity of the depression. The simplicity of the one process fits it for being useful in many situations, (in an astronomical

observatory for instance) where the complication attending the other would render it quite inapplicable.

5. If we use the numbers formerly noticed for a and λ, and put $w = 2{\cdot}3$ troy grains; then $\frac{48\,a}{\lambda} = {\cdot}0233$, or $\frac{1}{40}$ nearly; and $\frac{48\,a\,w}{\lambda} = {\cdot}26$, or $\frac{1}{4}$ nearly; and the formulæ (2) and (3) will become

$$x = \varrho - \frac{\delta\tau}{40},$$

$$y = m - \frac{\delta\tau}{4}.$$

These expressions are very simple and easily remembered. If to the scale of the evaporating thermometer another scale were attached, showing the maximum tensions at the various degrees of temperature; or one showing the maximum quantities of humidity in a cubic foot at the same temperatures; we should only have to deduct $\frac{1}{40}$ of the observed depression to have the actual tension sought, or $\frac{1}{4}$ of the same quantity, to have the actual quantity of vapour in a cubic foot. As $\delta\tau$ will always be confined to a few degrees, a small table containing the quantities to be subtracted might be engraved apart on the scale of the instrument.

The purpose of these observations is to explain the method proposed by Dr. Hutton for finding the moisture of the atmosphere on its own principles, and to disengage it from all mechanism foreign to it. The process requires nothing more than two thermometers which we have supposed to be constructed according to the centigrade scale. It is greatly to be wished that a number of experiments were made sufficient for ascertaining the practical fitness of the method for accomplishing its end.

J. Ivory.

XVII. *On a New Theory of the Tides.* *By Capt.* Forman, *of the Royal Navy.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—If the facts and arguments contained in the following letter are true, they must add to our stock of knowledge, and ought to be made known; if they are not, they can easily be refuted; and therefore, as in either case they can do no harm, I shall feel obliged if you will find room for them in your Magazine. I am, gentlemen, &c.

W. Forman.

To

To the Members of the Astronomical Society.

Gentlemen,—As you have thought it necessary, in a paper addressed to the public, to call the attention of astronomers to that branch of astronomy which is connected with the rising of the tides, it is evident, that in your opinion the old hypothesis is not quite satisfactory; or at least that you wish to be furnished, by those who have had the opportunities of witnessing their phænomena in different parts of the world, with such additional facts and documents as may either fully establish the old theory, or lead to the discovery of a better.

In obedience then to this call, I beg leave to submit to your judgement a Treatise on the Tides, which I have just written, and which, by taking in the compressibility of water as a co-operating principle, will account for several very important phænomena that cannot be explained in any other way. Thus, for instance, if the compressibility of water be admitted *in a sufficient degree*, as the moon's attraction (being in opposition to the earth's) must take off a portion of the gravity of every particle of water, these particles must necessarily expand in proportion to the weight that is taken off them; and, as the sum of the expansion of a great many particles must be a great deal more than the sum of the expansion of a few, we shall have a considerable rising in the deep parts of the ocean, without any sensible alteration in lakes and shallow water. Thus, then, if we admit the compressibility of water in a sufficient degree, we shall have a principle that will satisfactorily explain why the flood tide, in all places, always comes from the ocean; why there are no tides in lakes and some inland seas; why the tides do not rise to the same height in all places where the moon is equally vertical; and lastly, why other *loose* substances are not raised by the power of the moon's attraction as well as water. But how can we account for all this, if we deny the compressibility, and consequently the expansion, of water?

If the moon's attraction, co-operating with the centrifugal force of bodies, is capable of raising the tides in the ocean, why does it not also raise the waters in lakes and ponds? and why are not all other loose substances affected in the same manner? If the earth, as was supposed by Newton, is constantly drawn towards the moon by the power of her attraction, and the waters on the same side, by being so much nearer than the central parts of the earth, are made to move so much faster, and thus are raised above their ordinary level, why are not all other loose substances, that are equally near, raised in the same manner? There is only one answer can be given to these questions. None of these substances are elastic; but water is: and in proportion to its depth will be the degree of its expansion.

The compressibility of water to a certain extent has been proved by Mr. Perkins, and is now, I understand, admitted by all the philosophers. The only question then is, Whether it be compressible in a sufficient degree to produce the effects I have supposed. And, as no one can pretend to set bounds to the extreme depth of the ocean, or say to what degree water may be compressed in its deepest parts, no one can say that my hypothesis is impossible, or even improbable; while every one who is capable of thinking, when he considers that the moon's attraction has no power to lift the smallest or lightest substance that happens to be loose upon the surface of the earth, must be convinced that it could have no power to raise water, if it was not aided by some other principle; and surely there is no other principle that can aid in producing this effect, besides the elasticity of the particles of water.

Mr. Perkins, by a force which he considered equal to the weight of 320 atmospheres, or about two miles depth of water, compressed the water in a piezometer at the rate of three and a half per cent. In what manner the experiment was contrived I do not pretend to know; but, when we consider that water, in another experiment of his, forced its way into a bottle that was corked and sealed, it is certainly very possible that some of these minute particles might have escaped from the instrument before it was weighed, and thus the degree of compressibility may have been considerably underrated. This appears to me to be the more likely, because in another of Mr. Perkins's experiments the degree of compression was evidently a great deal more. In this experiment, the water, which had forced its way through the sealing of an empty porter bottle, at the depth of five hundred fathoms, on being drawn up and the external pressure in great measure diminished, expanded to such a degree that it forced the cork up against the coverings, compressed it into half its size, and then burst the bottle. As we have no means of measuring the degree of compression in this experiment, we must content ourselves with a rough estimate; but it is evident that it must have been equal to the space that was occupied by the cork before it was forced up, because the bottle burst afterwards; and this, at the lowest calculation, could not have been less than three per cent. when the force applied was very little more than the weight of one hundred atmospheres. A quart bottle is generally supposed to hold about a dozen common-sized wine-glasses; and if we suppose the space that was occupied by that part of the cork that was forced upwards to be the fourth part of a wine glass, it will make the expansion of the water, before the bottle burst, to be about two per cent., or the fiftieth part of the whole. This however is but a small part of the expansion; for, as the water forced its way in through, or rather under, the coverings, we must suppose that some of it escaped in the same way; and yet the force of the expansion still continued to such a degree, that, when the cork could no longer give way, it burst the bottle. Here then we have positive proof that the expansion of water, at the depth of only five hundred fathoms, was at least more than two, and in all probability was as much as four or five, per cent.; and who will say that the extreme depth of the ocean may not extend to three, four, or even five hundred miles*, and that the compression of water at those depths may not exceed even a hundred per cent.? It is not too much to suppose that the average of the compression of water in the deepest parts of the ocean, may be at least twenty-five per cent.; and, if we take the depth of the ocean at two hundred miles†, the expansion or rise of the waters at that depth, if the *entire* gravity of every particle was taken off, would amount to 50 miles or 48,000 fathoms; and if we divide this sum by 2400, which I take to be the amount of the power of the moon's attraction, we shall have a rising of the waters in the deep parts of the ocean equal to the height of 20 fathoms, and which is fully sufficient to account for all the phænomena connected with the rising of the tides.

Whether or not this estimate will be admitted by philosophers is of very little moment, since I have it in my power to prove, by *undeniable facts*, that the expansion of water is the immediate cause of the rising of the tides: and I beg leave here, gentlemen, to request your particular attention to the facts I am about to produce, and the inference I shall deduce from them.

During the time of flood tide, when the waters are rising, instead of showing any disposition to go *towards* the moon, they press downwards towards the earth's centre; which evidently proves that they are *pushed* upwards by the expansion of the particles below, and not *pulled* upwards by the power of the moon's attraction; for it is impossible to account for the tendency of the waters to press downwards, at the very moment that they are rising, without supposing a sufficient degree of expansion in the particles below to push them upwards; and therefore, however it may appear to contradict our experiments and preconceived opinions, the fact is indisputable.

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* If we may believe some of the geological writers, the ocean has no bottom at all, and the earth is nothing more than a crust formed upon its surface. This is a doctrine, however, I do not subscribe to.

† If we take the depth of the ocean at four hundred miles, a mean compression of twelve and a half per cent. will be sufficient for my purpose.

The same argument applies with equal force to the rising of the tides on the side opposite the moon; for, if the waters on this side were left behind, as Newton supposed, by the other parts of the earth being more strongly attracted by the moon in consequence of being so much nearer, or were carried upwards by a centrifugal force, they could have no tendency to press downwards; and yet we find that, at the very moment they are rising upwards, it is nothing but the resistance of the water below that prevents them from falling down towards the earth's centre. On this side then, as well as on the other, there must be an expansion of the particles of water to produce a rising of the tides; and, as the direction of the moon's attraction is here the same as the earth's, we can only account for this diminution of the weight or gravity of the particles of water on this side the earth, by supposing that the power of the moon's attraction takes off some portion of the power of the earth's attraction, in the same manner as I have shown the attraction of one magnet will diminish the attraction of another, when their two ends are of the same denomination.

You see then, gentlemen, that this is not a question of mere opinion: it is a question of facts, and is to be proved, not by supposing what the known degree of the compressibility of water *may* be capable of producing, but what it actually *does* produce. In short, we have only to take a handful of water out of the ocean, at the time of the rising of the tides, to be convinced that the expansion of water is the immediate cause of the phænomenon; because, if the waters were pulled upward by the power of the moon's attraction and not pushed upwards by the expansion of the particles below, this water would not fall back to the earth, until the influence of the moon's attraction had gone off.

Here then, gentlemen, upon these grounds I rest my case; and I call upon you, and, as I intend that this letter shall go forth into the world, I call upon every astronomer and every philosopher, to answer the following questions: Do the waters at the time of their rising, press downwards, or do they not? If they do, how are we to account for their rising, except by supposing that they are pushed upwards by the expansion of the particles below? If you can account for this in any other way, I am ready to confess that, as my hypothesis, in that case, will not be wanted, you will effectually deprive me of the only grounds upon which I can make good its claim to supersede the old one. But if you cannot, you must acknowledge that the expansion of water is the immediate cause of the rising of the tides; and then, as philosophers, as lovers of truth, as men whose judgement will be rejudged by a succeeding generation, that will know nothing of the bias and prejudices of the present day; but, above all, as men of integrity and honour, you are bound to admit so much of my theory as depends upon the expansion of water*.

In speaking of the bias and prejudices of the present day, I beg to be understood as merely meaning to infer, that opinions, which we have been taught in our infancy to revere as the most sacred truths, very frequently take so strong a hold upon the mind, that they are not easily eradicated even when they can be proved to be wrong; to venture to dispute them is little short of being guilty of a crime, and we shut our ears against every argument by which they can be oppugned. It occasionally happens, however, that the truth of some one of these dogmas, especially when it is grounded upon no better argument than authority, is called in question: this produces discussion, the struggles of its advocates hasten its downfall; and though we may still obstinately cling to it with the tenacious adherence of drowning men, it loses its hold upon posterity, and a succeeding generation, considering it merely in the light of a debatable question, judges of it by its intrinsic merits alone, and, throwing aside all prejudice, makes it give place to a better system. It is thus that opinions, which in the time of Galileo were held to be indisputable, are now universally exploded even by the most ignorant.

I am, gentlemen,
Your most obedient and very humble servant,
WALTER FORMAN.

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* Let us suppose a man to be carrying a weight upon his shoulders, and occasionally to be stooping down, and standing upright. Would not the *continued* pressure of the weight upon the man's shoulders, be a sufficient argument that it was not *pulled* upwards by an invisible power above, but was *pushed* upwards by his own exertions? and are not both cases precisely similar? If we could, by any possibility, take away the water below, can any one believe that the power of the moon's attraction could prevent the water above from falling down? and, if it could not, how can we account for the rising of the tides, except by supposing expansion in the particles of water?

XVIII. *True apparent Right Ascension of Dr.* MASKELYNE'S *36 Stars for every Day in the Year* 1822, *at the Time of passing the Meridian of Greenwich. By the Rev.* J. GROOBY.

[Continued from page 53.]

True apparent Right Ascension of Dr. Maskelyne's 36 Stars.

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	9·17	14·09	3·27	47·02	38·21	2·52	6·99	35·65	20·66	16·97	1·59	27·98	52·28	55·20	0·32	27·34	51·46	34·40
2	17	10	29	05	25	55	7·02	68	69	17·01	62	28·01	30	22	34	36	46	39
3	18	12	31	08	29	58	05	71	72	04	65	04	32	25	35	37	46	39
4	18	13	33	10	33	60	09	74	75	08	68	08	35	27	36	38	46	38
5	18	15	35	13	37	63	12	77	78	11	71	11	37	29	37	39	47	38
6	19	17	37	16	41	66	15	80	81	15	74	14	40	31	38	40	47	37
7	19	19	39	19	45	69	18	83	84	19	77	18	42	33	40	42	47	37
8	19	20	41	21	49	72	21	86	87	22	80	21	45	35	42	43	48	37
9	19	21	42	24	53	74	24	89	90	26	83	25	48	38	43	45	48	37
10	19	23	44	26	57	77	27	92	93	29	86	28	50	40	45	46	49	37
11	19	24	46	29	61	79	31	95	96	33	89	31	53	42	47	48	49	37
12	19	25	48	32	65	82	34	98	99	36	92	35	56	45	48	49	50	37
13	19	27	49	34	68	85	37	36·00	21·02	40	95	38	58	47	50	51	* 50	37
14	19	28	51	37	72	87	40	03	05	43	98	42	61	50	52	53	51	37
15	19	29	53	39	76	90	43	06	08	47	2·01	45	63	52	54	54	52	37
16	19	30	54	42	80	93	47	09	11	51	04	48	66	55	55	56	52	37
17	19	32	56	45	84	96	50	12	14	55	07	52	69	58	57	58	53	37
18	19	33	58	47	87	98	53	15	17	58	10	56	72	60	59	60	54	37
19	19	34	56	50	91	3·00	56	18	20	62	13	59	75	63	60	62	55	38
20	19	35	61	52	94	03	59	20	23	65	16	63	77	65	62	64	56	38
21	19	36	62	55	98	05	62	23	26	69	20	66	80	68	64	66	58	38
22	19	37	63	57	39·02	08	65	26	29	72	23	70	83	71	66	68	59	39
23	18	38	65	59	05	10	68	29	32	76	26	73	86	74	68	70	60	39
24	18	39	66	62	09	13	71	32	34	80	29	76	89	76	70	72	62	39
25	18	40	68	64	12	15	74	34	37	83	32	80	92	79	72	74	63	40
26	17	41	69	66	16	18	77	37	40	87	36	83	95	82	74	76	64	40
27	17	42	71	69	20	20	80	40	43	91	39	87	98	85	76	78	65	41
28	17	43	72	71	23	22	83	42	46	94	42	90	53·01	88	79	80	67	42
29	16	43	73	73	26	25	86	45	49	98	45	94	04	91	82	83	68	43
30	16	44	75	75	30	27	88	48	51	18·01	49	97	07	94	83	85	70	44
31	15	45	76	78	33	29	91	50	54	05	52	29·01	10	97	86	88	72	45

1822.	1α Libræ	2α Libræ	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1α Capri.	2α Capri.	α Cygni	α Aqua.	Fomalhaut	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Oct.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	53·67	5·09	10·96	32 55	44	34·40	42·90	56·73	51·00	9·12	37·52	50·45	14·22	24·63	42·40	62	58·14	16·61
2	66	08	95	54	43	38	88	71	50·98	10	50	43	20	60	39	61	14	61
3	65	07	94	53	41	36	86	68	97	08	48	42	19	58	38	60	14	61
4	65	07	92	52	40	35	85	66	95	07	47	40	17	56	37	60	13	62
5	64	06	91	51	39	33	83	63	93	05	45	39	16	54	37	59	13	62
6	64	06	89	50	38	32	81	61	92	04	44	37	14	51	36	58	12	62
7	64	06	88	49	36	30	80	58	90	02	42	36	13	49	35	57	12	62
8	63	05	87	49	35	29	78	56	89	00	40	34	11	47	34	56	11	62
9	63	05	86	48	33	27	77	53	87	8·99	39	33	10	44	33	56	11	62
10	63	05	85	47	32	26	75	51	86	97	37	31	08	42	32	55	10	62
11	63	05	84	46	31	24	74	48	84	96	36	30	07	39	31	54	10	62
12	62	04	83	46	29	23	72	46	83	94	34	28	05	37	30	53	09	62
13	62	04	82	45	28	21	71	44	82	92	32	27	04	34	29	52	08	62
14	62	04	81	44	27	20	69	41	80	91	31	25	02	32	28	52	08	62
15	62	04	80	43	26	18	68	38	79	89	29	24	01	29	27	51	07	62
16	62	04	79	43	25	17	66	36	77	88	28	22	13·99	27	26	50	07	62
17	62	04	79	42	24	15	65	34	76	86	26	21	98	24	25	50	06	62
18	62	04	78	42	23	14	64	31	74	84	24	19	96	22	24	49	05	62
19	63	05	78	41	22	12	62	29	72	83	23	18	95	19	23	48	04	61
20	63	05	77	41	21	11	61	27	71	81	21	16	93	17	21	47	04	61
21	63	05	77	40	21	10	60	25	69	80	20	15	92	14	19	46	03	61
22	63	05	76	40	20	09	58	22	67	78	18	13	90	12	18	45	02	61
23	64	06	75	40	19	08	57	20	65	76	16	11	88	09	17	44	01	60
24	64	06	75	39	19	07	56	18	63	75	15	10	87	07	16	43	00	60
25	64	06	74	39	18	06	55	15	62	73	13	08	85	04	15	42	00	60
26	65	07	74	38	18	05	53	13	60	72	12	07	84	02	14	41	57·99	59
27	65	07	73	38	17	04	52	11	58	70	10	05	82	23·99	13	39	98	59
28	66	08	73	38	17	03	51	09	56	69	09	04	81	97	11	38	97	58
29	67	09	73	38	17	02	50	07	55	67	07	02	79	94	10	36	96	58
30	67	09	73	38	16	02	49	05	53	66	06	01	78	92	09	35	95	57
31	68	10	73	38	16	01	48	03	52	64	04	49·99	76	89	08	33	94	57

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Nov.	s.	s.	s.	s.	s	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	9·15	14·45	3·77	47·80	39·36	3·32	7·94	36·53	21·57	17·09	2·55	29·05	53·13	56.00	0·88	27·90	51·73	34·46
2	14	46	78	82	40	34	97	56	60	12	58	08	16	03	90	92	75	47
3	14	47	79	84	43	36	99	58	63	16	61	11	19	06	93	95	77	48
4	13	47	80	86	46	38	8·02	61	65	19	64	15	22	09	95	97	78	49
5	13	48	81	88	49	41	05	64	68	23	67	18	25	12	97	28·00	80	50
6	12	49	82	90	53	43	08	67	71	27	70	22	28	15	1·00	02	82	51
7	11	49	83	92	56	45	11	69	74	18·30	73	25	31	18	03	05	84	52
8	10	49	84	94	59	47	13	72	77	34	76	29	34	21	05	08	86	53
9	10	50	85	96	62	49	15	74	79	37	79	32	37	24	08	10	88	55
10	09	50	85	98	65	51	18	76	82	41	82	36	40	27	11	13	90	56
11	08	50	86	48·00	68	53	20	79	85	44	85	39	43	30	14	16	92	57
12	08	51	87	02	70	55	23	81	87	47	88	42	47	34	17	19	94	59
13	07	51	88	03	73	57	25	84	90	51	91	46	50	37	20	21	96	60
14	06	51	88	05	76	59	28	86	92	54	94	49	53	40	22	24	98	62
15	06	51	89	07	79	61	30	88	95	58	97	52	56	43	25	27	52·00	63
16	05	52	90	09	82	63	33	91	98	62	3·00	56	60	47	28	30	02	65
17	04	52	90	11	85	65	35	93	22·01	65	03	59	63	50	31	33	04	66
18	03	52	91	12	88	66	37	95	03	69	06	63	66	53	34	36	07	68
10	03	54	91	13	90	68	40	97	06	72	09	66	69	56	37	39	09	70
20	02	53	92	15	93	70	42	37·00	08	75	12	69	73	60	40	42	12	72
21	01	53	92	16	95	71	44	02	11	79	15	73	76	63	43	45	14	74
22	00	53	93	18	98	73	46	04	13	82	18	76	79	66	46	48	17	76
23	00	53	93	19	40·00	74	49	06	15	86	21	79	83	69	49	51	20	78
24	8·99	53	94	21	03	76	51	08	18	89	23	83	86	73	52	54	22	80
25	98	53	94	23	05	77	53	11	20	92	26	86	89	76	55	57	25	82
26	97	53	95	24	08	79	55	13	23	96	29	90	93	80	58	60	28	84
27	96	53	95	26	10	81	57	15	25	99	32	93	96	83	61	63	31	86
28	95	53	95	27	12	82	59	17	27	19·02	34	96	99	86	65	66	34	89
29	94	53	96	28	15	83	61	18	29	05	37	99	54·02	90	68	69	37	91
30	93	53	96	29	17	85	62	20	32	08	40	30·02	05	93	71	72	39	93

1822.	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis.	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri.	2 α Capri.	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Nov.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	53·68	5·10	10·73	32·38	33·16	33 00	42·47	56·01	50·50	8·63	37·03	49·98	13·75	87	42·07	52·32	93	56
2	69	11	73	38	16	99	46	55·99	49	61	02	97	74	84	06	31	92	56
3	70	12	73	38	15	98	45	97	47	60	00	95	72	82	04	29	91	55
4	71	13	73	38	15	97	44	95	46	58	36·99	94	71	79	03	28	90	55
5	* 72	* 14	73	39	15	97	43	93	44	57	97	92	69	77	02	26	89	54
6	73	15	73	39	15	96	43	91	43	55	96	91	68	74	01	25	88	53
7	75	17	74	40	16	96	42	89	42	54	95	90	67	72	41·00	24	87	52
8	76	18	74	40	16	95	42	88	40	53	93	88	65	69	98	22	86	51
9	77	19	74	41	16	95	41	86	39	51	92	87	64	67	97	21	85	51
10	79	21	75	41	16	94	41	84	38	50	91	86	63	64	96	19	84	50
11	80	22	75	42	17	94	40	82	36	49	90	84	61	62	94	18	83	49
12	82	24	76	43	17	94	40	81	35	48	88	83	60	59	93	17	82	48
13	83	25	76	43	17	93	40	79	34	46	87	82	59	57	92	15	81	47
14	84	26	77	44	18	93	39	78	33	45	86	81	58	54	91	14	80	47
15	86	28	78	44	18	92	39	76	31	44	85	79	56	52	89	12	78	46
16	87	29	* 78	45	19	92	39	75	30	43	84	78	55	49	88	11	77	45
17	89	31	79	45	20	92	39	74	29	42	83	77	54	47	87	09	76	44
18	91	33	80	* 46	20	92	38	73	28	41	82	76	53	45	86	08	75	43
19	93	35	81	48	21	92	38	71	27	40	81	75	52	43	85	06	74	42
20	94	36	82	49	22	92	38	70	26	39	80	74	51	41	83	05	72	41
21	96	38	83	50	23	92	38	69	25	38	79	73	50	38	82	03	71	40
22	97	39	84	52	24	92	38	68	24	37	78	72	49	36	81	02	70	39
23	99	41	85	53	25	92	38	66	23	36	77	71	48	34	80	01	69	38
24	54·01	43	86	54	26	93	38	65	22	35	76	70	47	32	78	99	67	37
25	03	45	87	55	27	93	38	64	21	35	75	69	46	30	77	98	66	36
26	05	47	89	57	28	93	38	63	21	34	74	69	46	27	76	96	65	35
27	07	49	90	58	29	94	38	62	20	33	73	68	45	25	75	95	64	34
28	10	52	92	60	* 31	94	39	62	20	33	73	67	44	24	74	93	63	33
29	12	54	93	61	33	95	39	61	19	32	74	67	44	22	73	92	62	32
30	14	56	95	62	34	95	40	60	19	31	74	66	43	20	71	90	61	30

1822.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran	Capella.	Rigel.	β Tauri	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Dec.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	8·92	14·52	3 96	48·30	40·19	3 86	8·64	37·22	22·34	19·11	3·42	30·05	54·09	56·96	1·75	28·76	52 42	34 95
2	91	52	97	31	21	88	66	24	36	14	45	08	12	57·00	78	79	45	98
3	90	52	97	32	23	89	68	25	38	17	48	11	15	03	81	82	48	35·00
4	89	52	97	33	25	91	69	27	40	20	50	14	18	07	84	85	50	02
5	88	51	97	35	27	92	71	29	43	24	53	17	21	10	88	88	53	05
6	87	51	98	36	29	94	73	31	45	27	56	20	25	14	91	92	56	07
7	86	50	98	37	30	95	75	33	47	30	58	23	28	17	94	95	59	10
8	85	50	97	38	31	96	76	34	48	33	61	26	31	20	98	99	62	12
9	84	49	97	38	33	97	78	36	50	35	63	29	34	24	2·01	29·02	65	15
10	83	49	97	39	34	98	79	37	52	38	66	32	37	27	04	06	68	18
11	82	48	97	40	36	99	80	39	54	41	68	35	40	30	08	09	71	21
12	81	47	97	41	37	4 00	82	41	56	44	71	37	43	34	11	12	74	24
13	80	47	97	42	39	01	83	42	57	46	73	40	46	37	15	16	77	27
14	79	46	97	43	49	02	85	44	59	49	75	43	49	40	18	19	81	30
15	78	46	97	43	42	03	86	45	61	52	78	46	52	44	21	22	84	32
16	77	45	97	44	44	04	88	47	63	55	80	49	55	47	25	26	88	35
17	76	44	96	45	45	05	89	48	64	57	82	51	58	50	29	29	91	38
18	75	43	96	45	46	05	90	49	65	60	84	54	61	53	32	33	94	41
19	74	43	96	46	47	06	91	51	67	62	87	56	64	57	35	36	98	44
20	73	42	95	46	48	06	92	52	68	65	89	58	67	60	39	40	53 01	47
21	72	41	95	46	49	07	93	53	70	67	91	61	70	64	42	43	04	50
22	71	40	95	47	50	07	94	54	71	69	93	63	73	67	45	46	08	53
23	70	40	95	47	51	08	95	55	72	72	95	65	76	70	49	49	11	56
24	69	39	94	47	52	08	96	56	74	74	97	68	78	73	52	53	14	59
25	67	38	94	48	52	09	97	57	75	77	99	70	81	76	55	56	17	62
26	66	37	93	48	53	09	97	58	77	79	4·01	72	84	79	59	60	21	63
27	65	36	93	49	54	09	98	59	78	81	03	74	87	83	62	63	24	67
28	64	35	92	49	54	10	99	60	79	83	05	76	90	86	65	66	27	70
29	63	34	92	49	55	10	9·00	60	80	85	06	78	92	89	69	70	31	73
39	62	33	91	49	55	10	00	61	81	87	08	80	95	92	71	73	34	76
31	61	32	90	49	56	11	01	62	82	89	10	82	98	95	74	76	37	80

1822.	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri	2 α Capri.	α Cygni.	α Aquæ.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Dec	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	54·16	5·58	10·96	32 64	33·36	53 96	42 40	55 59	30 18	8·30	36.73	49·65	13·42	18	41·70	52 89	59	29
2	18	60	98	65	37	97	40	59	17	30	72	65	42	16	69	88	58	28
3	20	62	99	67	39	97	41	58	17	29	71	64	41	14	68	86	57	27
4	22	64	11·01	69	41	98	41	57	15	28	70	63	40	12	67	85	56	25
5	25	67	02	71	42	99	42	56	16	28	69	63	40	10	66	83	55	24
6	28	70	04	73	44	99	42	56	15	27	68	62	39	08	65	82	54	23
7	31	73	06	75	46	54 00	43	55	15	27	68	62	39	06	64	81	53	22
8	33	75	08	77	18	01	44	55	14	27	67	61	38	05	63	79	52	20
9	36	78	10	79	50	02	45	55	14	26	66	61	38	03	62	78	51	19
10	39	81	12	81	52	04	46	55	14	26	66	61	38	02	61	76	49	18
11	42	84	14	83	54	05	46	54	13	26	66	60	37	00	60	75	48	16
12	44	86	16	85	56	06	47	54	13	26	66	59	36	98	59	74	47	15
13	47	89	18	87	58	07	48	54	13	25	66	59	36	97	58	72	46	14
14	51	93	20	89	60	08	49	54	13	25	65	59	36	95	57	70	45	13
15	53	95	22	91	62	09	51	54	12	25	65	59	36	94	56	69	44	11
16	55	97	25	93	64	11	52	54	12	25	65	59	36	92	56	68	42	10
17	58	6·00	27	95	66	12	53	55	12	25	65	59	36	91	55	67	41	09
18	61	03	30	98	69	14	55	55	12	25	65	59	36	90	54	66	40	07
19	64	06	32	33·00	71	15	56	55	12	25	65	59	36	89	54	65	39	06
20	67	09	35	03	74	17	58	56	12	25	65	59	36	88	53	63	38	05
21	70	12	37	05	76	18	59	56	12	25	65	59	36	86	52	62	37	03
22	73	15	40	08	78	20	60	57	12	25	65	59	36	85	52	61	36	02
23	76	18	42	10	81	22	62	57	13	25	65	59	36	84	51	60	35	01
24	79	21	45	13	83	23	63	58	13	25	65	59	36	83	50	58	34	00
25	82	24	47	15	86	25	64	58	13	25	66	59	36	81	50	57	33	98
26	85	27	50	18	88	27	66	59	14	26	66	59	36	80	49	56	32	97
27	88	30	52	20	91	29	67	60	14	26	67	59	36	79	49	55	31	98
28	91	33	55	23	94	31	69	61	15	26	67	60	37	78	48	54	30	96
29	94	36	57	26	97	33	71	63	15	27	67	60	37	77	48	53	29	95
30	97	39	60	28	34·00	35	73	64	16	27	68	60	37	77	47	52	28	94
31	55·00	42	63	31	02	37	75	65	16	28	68	61	38	76	47	51	27	92

XIX. *On Chlorine and Chlorate of Potassa.* *By* JOHN MURRAY, *F.L.S. M.G.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

July 8, 1822.

GENTLEMEN, — SINCE my last communication I have made a few experiments in reference to the sensation of heat developed by immersing the hand in chlorine. As far as can be ascertained by this means, the increment of temperature certainly exceeds 100° F. The temperature of the atmosphere being 66° F., the thermometer was plunged into a volume of chlorine diluted with atmospheric air, and into the same gas very pure, and intense in its characteristic colour; but the thermometer indicated no decided change, or at any rate the average of several experiments did not exceed ·5 F.:—still the sensation of heat on the skin was unequivocal. The ball of the thermometer was then *wetted*, but no perceptible change occurred. In these experiments the vessels of chlorine were exposed at top to atmospheric influence. The hand was passed up into the chlorine *over the shelf* of the pneumatic cistern, and a similar increase of temperature experienced, while the thermometer *under the same circumstances* stood at 66°.

I have said that this interesting property is not exclusively peculiar to chlorine, and instanced *nitrous gas* when opened in contact with atmospheric air. The temperature of the external air was 67° F. and the thermometer rose in the nitrous acid gas thus formed to 68°; in other experiments the external temperature being 63·5. The thermometer rose to 65° F. But in another vessel, the ball of the instrument being *moistened*, the mercury rose to 70° F., though the temperature felt by immersion of the hand into this gas would be unhesitatingly pronounced to be above 100° F. In the latter case the moistened ball showed an increment over the instrument when dry, but this refers entirely to their relative absorption by water, and consequent condensation,—and has evidently nothing to do with their reciprocal action on the *epidermis*.

It is known that if sulphuric acid be passed through water on the chlorate of potassa mixed with chips of phosphorus, the curious and beautiful phænomenon of combustion under water ensues; but I find that *flashes of light* are exhibited by merely bringing the sulphuric acid in contact with the chlorate, although phosphorus is introduced. In this case *chlorine* is disengaged, while the supernatant fluid acquires a yellow tinge, and in smell and other properties resembles a solution of protoxide of chlorine. Gold-leaf does not dissolve in it, but vegetable blue colours are discharged.

I made some experiments on the orange-coloured vapour arising from the action of sulphuric acid on chlorate of potassa. I shall not advert to any repetition on my part of experiments made by others on this vapour,—merely stating, *en passant*, that *tallow* and *caoutchouc*, though they have been pronounced successful in this case, did not, at any rate, succeed with me; and even *croton oil* and *cocoa-nut oil* were equally unsuccessful.

Potassium ignited with brilliant *jets de feux*; yet *sodium* did not ignite, the metal being merely expelled with explosion.

Heated charcoal (*from the betel-nut*) exploded with light, and *morphia* exhibited a flash of greenish light.

Chloric ether, *phosphorized alcohol* and *ether*, and *sulphuret of carbon*, inflamed with explosion.

Innulin exploded with light; and *phosphoret of lime* kindles and explodes.

Benzoic acid and *naphthalin* explode with beautiful combustion.

Sulphur not previously heated, as well as a mixture of *selenium* and *sulphur*, exploded in this vapour without light.

Both *artificial camphor* and *deutochloride of phosphorus* were perfectly inert.

Allow me, gentlemen, now to add, that chlorate of potassa has been on my recommendation introduced into the practice of several physicians with the greatest success.

In *active* and *passive hæmorrhage* and in the *synochus*, as well as *dyspepsia*, its exhibition has been attended with most decided benefit and the happiest results.

Five doses of five grains each operated a cure in hæmoptysis: it was administered twice *per diem*, morning and evening. Where in dyspepsia an irritability and excitement obtain, and calomel is rejected, a dose of four grains of the chlorate of potassa has been attended with the most marked advantage. Perhaps this quantity administered once every second day will be found sufficient, and the quantity may be abridged in particular cases, as in delicate constitutions or idiosyncrasy.

I have in my own case dissolved two drachms in a pint of distilled water, and taken two table spoonfuls twice a day, say morning and evening one. The chlorate of potassa seems to promise benefit in scrofula.

I was led to recommend this salt in *internal hæmorrhage*, from having experienced its admirable effects on myself, in a case the consequence of a violent fall which I experienced.

I may presume the liberty to quote from the letter of a friend, a physician of considerable practice, respecting its employment on my recommendation.

"I am happy to have it in my power to give you a most favourable account of the oxymuriate of potassa.

"I have given it in several cases of both active and passive hæmorrhage with the best effects, as well as in the synochus. It seems to have the very speedy effect of lowering the pulse, and as much so as the digitalis, but with this difference:—so far from reducing the strength of the system, it, on the contrary, improves it. I have also given it in two cases of indigestion, arising from a torpid action of the liver, with decided advantage. I intend to persevere in the use of it."

I have the honour to be,
Gentlemen,
Your obliged and very obedient humble servant,
J. MURRAY.

XX. *An Account of the Repeating Circle, and of the Altitude and Azimuth Instrument; describing their different Constructions, the Manner of performing their principal Adjustments, and how to make Observations with them; together with a Comparison of their respective Advantages.* By EDWARD TROUGHTON, *Esq. F.R.S. and Member of the American Philosophical Society.*

[Concluded from p. 18.]

Manner of using the Altitude and Azimuth Circle.

IN geodesy this instrument, being adjusted, measures without further trouble angles between objects upon the horizontal plane, whatever may be the number required to be taken at one station. The parts of the instrument are all concentric, and therefore whatever it gives, whether the objects are remote or near, requires no correction. The angles having been read off individually will be vitiated by the errors of division; and even in graduation that may be deemed good, those errors may be too great in some cases that occur. Such cases are well known to the judicious surveyor, and may be obviated by simply turning the whole instrument upon the stand, and setting the axis vertical; again and again taking the angles which he wishes to ascertain with the utmost accuracy: for, by this expedient, he will get any angle upon as many different parts of the circle as he pleases or thinks necessary. This may at first view appear to be a principle derived from the other instrument; but let it be remembered that it is no more than taking *means*; a thing practised and well understood before the repeating instruments were brought into existence.

In astronomy the azimuth circle is of little use, except in furnishing ready means of bringing the upper circle into the plane of the meridian; it has however been used for making out, in conjunction with the other circle, the quantity of refraction at different altitudes; but as, perhaps, the times for this purpose furnish better data than azimuths, to mention the matter is sufficient. Observing altitudes of the celestial bodies is a thing so familiar to every one who is in the least acquainted with these subjects, that to describe it would only be to lengthen this paper. I may however remark that in meridian altitudes, there is time to get an observation with the face of the circle to the east, and another with its face to the west; which together give a collimated double result, before the diminution in altitude becomes sensible. Yet I feel somewhat diffident about recommending this mode of observing, although I have practised it myself with success. An observer, before he attempts it, should be expert both in managing the instrument and reading off the angles. Respecting stars near the equator, there is only about one minute of time on each side of the meridian, that the star would continue to be bisected by the wire of a telescope that magnifies sixty times: therefore, before the double observation is attempted, the time should be accurately known. In truth, all hurried operations with this instrument may be avoided and left to the repeating circle.

Comparison of the two Instruments.

I come now to compare the two instruments with each other in their application to the different purposes for which they are designed; and to prevent the frequent recurrence of long names, I shall call the repeating circle R, and the altitude and azimuth instrument A: an expedient which, perhaps, if sooner adopted, might have improved this paper.

To find the difference of latitude between distant places is a most important problem in extensive geodetic operations, and for this purpose R has often been represented as equal or even superior to the zenith sector: but as the latter instrument has always been constructed with a powerful telescope, and is in its nature the most simple possible, I beg leave to dissent from that opinion. For, however an instrument may be constructed, or in whatever manner it may be used, I have no faith that it can give results nearer the truth, than a quantity that is visible in the telescope. To do this, an instrument, with respect to any error that is not corrected by reversion, must be perfect; a thing of which I have no idea. Yet every one is not of my opinion. As an instance of this, a celebrated astronomer, a few years ago, in the south of Europe, made observations for finding the attraction of a mountain with a small instrument of the construction R; and obtained a deflection of the level equal to two seconds; and, although his telescope could not have been more than 15 inches long, from this experiment brought out a density of the earth nearly coinciding with the Schehallien experiment, and with the more recent one which Cavendish obtained by direct attraction. Yet has that same astronomer, from later experiments, found the *fixed error* of the same or a similar instrument (made by a foreign artist by no means unknown to fame) to amount to from five to ten seconds. Should this error, however, which is called fixed, turn out to be at all fortuitous, it is possible that, taking five for its amount (to say nothing about ten), a result might have been obtained of an equal quantity contrary to attraction. But on the other hand, were the error of such an instrument absolutely fixed, although it gave the altitudes incorrectly, yet might it give the differences correctly; and consequently the above dwarfish experiment might be permitted to stand on its own little base. It is a circumstance little suspected, and not very easy to explain, how an instrument, that is capable of reversion, can have errors that are not correct in the double result. But although this, like the above statement, and the remarks I have made upon it, may appear digressive, yet will it not be altogether useless to inquire into the nature of two of the most obvious sources of error, to which this instrument in particular, and others in a less degree are subject.

It is not perhaps so well known as it ought to be, either to observers or artists, that the air-bulb of the spirit level changes its position with a difference of temperature. This, it is probable, is wholly occasioned by the glass tube (I mean from its expansion) being larger at one end than at the other: for I have observed that as the temperature increases, the bubble always deviates towards the larger end, and as it diminishes, the deviation takes place the contrary way. The error occasioned by this cause, whatever it may amount to at the end of a series of repetitions, will be divided, and affect the result by no more than the mean deviation belonging to a pair of observations. This kind of error is fixed, as far as a single course of repetition is concerned; but, when the change of temperature is reversed, an error equally fixed will affect observations in the opposite direction.

Another and still more fatal source of error to which R is peculiarly liable, arises from the resistance of the centre-work to the action of the tangent-screw. This will be more or less, according to the care and judgement that have been employed in the construction of the instrument: but to bring it to nothing must for ever be beyond the skill of the artist, because it lies in some measure in the nature of the materials. I will now try to explain how this source of inaccuracy affects observation, and to this end will suppose that an observer, when he bisects a star, moves the index with the tangent screw, so as to advance it on the graduated limb according to the numbering of the degrees; the telescope indeed will be properly pointed, but the index will show too much. On reversing the position of the instrument, the telescope will naturally relieve itself from the friction at the centre, and take the position due to the index, instead of retaining that which it had when the star was bisected; therefore the following motion of the telescope will begin from a wrong point; and if the screw is always turned in the same direction, will produce an error in excess at every pair of observations. As much may be said respecting the motion of the level, which, on account of its socket embracing the exterior of the axis, will meet with a greater resistance than the telescope did. As far as these two parts of repetition are concerned, it is clear that the habits of an observer may make this source of error either fixed or accidental, and by constantly turning the screws in a particular way, he may have for a fixed error either their sum or their difference. There is another motion equally connected with the operation of repetition, and quite as liable to this kind of error; namely, the general one, which carries round together the circle, the telescope and level. To follow up these three sources of error, and show how in their different combinations a series of repetitions would be affected, is a task which I dare not attempt. Instead of which I will content myself with remarking, that the complicated centre-work of this instrument, and the different motions having a tendency to drag each other, together with the change of position that takes place without the different steps being registered, subject it to greater errors, when the graduation is tolerable, than those which it professes to correct. To examine whether the screws, which govern the three motions here treated of, give immediate motion to the respective parts which they act upon, is a good criterion, but I think incapable of detecting a quantity of error less than two or three seconds; which quantity, if it recurs as fixed, will not be divided in obtaining the general result, because it affects all the operations, and it is the accumulated error only that is reduced.

In the instrument A, the resistance of the centre-work is extremely small; the pivots of the transit axis merely resting on their angular supports, while the screw for slow motion acts immediately upon the circle, carrying also the telescope which makes one piece with it. This is the simplest action that an instrument can possess; the forces in opposition to each other are quite inconsiderable; there is no rubbing of indices; and the angles are read off immediately after observation without any thing having changed place. The error of the level, mentioned before, will also affect this instrument; but, by judicious management, as will be shown presently, may be almost wholly counteracted. The method indeed would apply to R, but at an expense of time that could not be admitted. It is this: reversing the position of the circle in azimuth, by the level, place the axis truly vertical, which then becomes a substitute for the level during the short time a star is bisected; after this observation is read off and the telescope set for another star, I would note the position of the level, and again reverse the instrument: when, if any deviation is seen, I would bring the level by the foot screw half-way to the point where it stood before. This operation places the axis vertical again, whether a deviation of the level or of the whole instrument has taken place. To do this in the easiest manner, is to make a series of observations with the divided side of the circle to the east and west alternately; this way saves half the trouble of frequent reversion, renders observations independent of each other, and makes it resemble in its use the zenith sector, as nearly as one instrument with a level can do a better with a plumb-line.

When A is used for ascertaining the value of a celestial arc, corresponding to a measured distance on the surface of the earth, many stars should be marked for observation, none of which ought to be more than 60° from the zenith; and to avoid hurried observation, they should be picked from a catalogue at nearly equal distances of right ascension. In doing this there can be no difficulty, because those of the fourth or fifth magnitude are for this purpose as good as any others. As to the time required for observing a star, different observers will want more or less, but surely the space of five minutes is enough for any one. If he has previously written down, from his catalogue, the degree and first figure of the minutes, he has no more to do in reading off than, when the star is bisected, to put down the last figure of the minutes and the seconds as he takes them from the micrometers. For the next star he should set to what his catalogue gives, then reverse the position of the instrument by the divisions of the azimuth circle, afterwards correct for any deviation of the level and verticality of the axis, and then wait for the appulse of the star to the meridian. In every pair of observations, the place of a star is read off on four points of the circle; and, where those marked for observation extend to 60° of zenith distance both to the north and south, 240° of the circle are employed. To extend it further could produce no good effect, for the uncertainty of refraction would counteract the advantage arising from a greater range. In using A, if an observation is hurt by a passing cloud, or any other circumstance that may render it doubtful, it had better not be written down, even should it agree with the general result; and, on the other hand, if the observer is satisfied with an observation, it should not be omitted for being somewhat discordant. With R, if any one of the steps of a series of repetition is not well performed, the whole will be vitiated should the observer go on. He may perhaps be able to save what has gone before, by reading off what has been done, and beginning a new set; but this cannot be done in all cases. I remember an instance, where, in a series of twenty double observations on *polaris* (that had been made with extraordinary care), by the mistake of the assistant, in the very last of them, by his turning the screw of the indices instead of that of the level, the whole labour was irretrievably lost.

When an observer is so circumstanced as not to be able to stay at a place more than a short time, R seems to have the advantage; for, in a few hours a series of repetitions may be made on *polaris* to answer his purpose; since the polar distance of that star is now so well known, that little will be lost by not having opportunity of observing it both above and below the pole. A is not, however, without its resources in this respect; for in stars that have not more than 25° of polar distance, there will be quite time enough for reversing the position of the instrument, and obtaining collimated results. I must here remark that this can only be done in high latitudes, because near the equator, the slow moving stars are too much affected by variable refraction to be relied on in nice matters. This last remark must be placed very much to the discredit of R, which in its nature is adapted to the slow moving stars alone.

Thus it has been shown that, respecting its most effective operations, the repeating circle is local with regard to both the heavens and the earth. It must be granted that at every step, even when a quick moving star is observed, if the time has been noted, the whole may be reduced to the meridian: and were it wanted, which it is not, I should claim the same concession in favour of A. But let it be remembered, that at any considerable distance from the meridian, the time becomes so important a datum, that it requires an exactness, scarcely to be found, except in a well regulated observatory. Therefore, in observing stars near the equator with R, the observer has the choice of two difficulties; he may either extend his observations to a distance on both sides of the meridian to obtain the requisite number of repetitions, incurring the error arising from uncertainty of time; or keeping close to the meridian, put up with a limited range, and render his work equally unsatisfactory through that circumstance. It was before stated that, to avoid hurried observation in the use of A, stars at nearly equal distances of right ascension should be chosen; and it should be remarked, that it is almost equally important to have them also at nearly equal distances of declination; because the more regularly they are dispersed over the whole range of arc employed, the more perfect will be the correction of erroneous dividing: for two stars having the same declination will evidently be affected by the same error, and if they differ only a little, will probably partake of the same. When A is used in the observation of many stars, the errors both of observation and graduation are reduced by taking the mean. With R, the errors of many observations on one star are diminished precisely in the same degree, but as the errors of the whole intermediate arc are passed over and do not enter at all into the account, only those of the zero reading and the final one are charged upon the general mean. This distinction, certainly in favour of R, may be gathered from what has been said before, and is the *little all* of R. It is here brought forward again, for the use of those numerous observers who have not seen through the blind process of repetition, and who have attributed to this instrument powers of approximating towards the inaccessible point of truth little short of the miraculous.

I will now subjoin in detail the observations of some zenith distances of a fixed mark made about ten years ago with an 18 inch repeating circle at St. John's College Cambridge, by a gentleman who ranks high in science at that distinguished university, and who is a most worthy member of this society.

Double Obs.		Zenith dist. by repetition.			Zenith dist. by successive pairs.
		° ′	″		″
1	=	92 45	3,2	——	3,2
2	=	——	4,8	——	6,3
3	=	——	6,3	——	9,4
4	=	——	6,8	——	8,3
5	=	——	7,5	——	10,4
6	=	——	6,9	——	4,0
7	=	——	7,2	——	8,7
8	=	——	6,9	——	5,2
9	=	——	7,1	——	8,7
10	=	——	7,0	——	5,9
11	=	——	7,2	——	8,7
12	=	——	6,8	——	3,1

Since writing the above, I have put my hand upon another experiment of the same kind, which was made to determine the angular distance between two land objects. It was made at Glasgow by a gentleman whose science is universally acknowledged, and who was eminently qualified to do justice to the subject. The diameter of the instrument was 12 inches.

Double Obs.		Angle by repetition.				Angle by successive pairs.
		°	′	″		″
1	=	63	33	52,5	——	52,5
2	=	——		51,0	——	49,5
3	=	——		50,0	——	48,0
4	=	——		48,7	——	45,0
5	=	——		49,3	——	50,0
6	=	——		49,1	——	50,0
7	=	——		48,6	——	45,0
8	=	——		48,5	——	45,0
9 10	=	——		48,8	——	51,5

The two series of observations with R, inserted above, are all I can find where the collimated angles have been read off. They serve to show, in such instruments as were used, the amount of the errors of observation, division and reading off, that one collimated result leaves to be corrected by repetition or taking means. Thus, in the first example, the greatest differences from the general mean are +3″,4 and −3″,7, and in the second, +3″,7 and −3″,8. The examples also exhibit the irregular manner in which the successive pairs differ from each other; while the columns of repetition with more regularity approximate towards a constant quantity, as the divisor of the total arc becomes greater. It may be useful sometimes, to exhibit a series of observations in this manner, for it shows what may be expected from an instrument after a certain number of repetitions. It is curious that, in both of the examples, the same result is obtained at the fourth pair as was gained at last, a circumstance, no doubt, purely accidental, and which could not have been known, had the ordinary means of reduction been employed.

With respect to the accuracy of the angles obtained in the two series of observations, it might be presumed, that in neither case, could they differ from the truth by a quantity much greater than half a second: but this presumption depends entirely upon the instrument being perfect, or free from fixed error; a thing, for the detection of which, an observer is not furnished with any direct means.

The instrument R has been equally praised by those who have used it for taking angular distances, as it has been in astronomy; and I have in this place but little to add to my former remarks. The observer is not here, as in astronomy, compelled to perform his operations in a short time: he may take as much as he pleases, and in truth he will want not a little. Even by the improved method, it requires much time to place the circle in the plane of the two objects: then follows the process of repetition; then that of observing the elevation or depression of the objects; and lastly, measuring, if they are near, or estimating, if remote, their distance from the station, to enable him to correct the angle for the effect of eccentricity of the telescopes. All this must be done before he has data for computing the horizontal angle; and all this is requisite for every angle that he takes; even from the same station. A gives the horizontal angle at once; its parts are perfectly concentric, and therefore the distance of the objects is not concerned: the motion of the telescope, like that of a transit, is truly perpendicular, therefore the elevation or depression of the objects comes not into the account. When the adjusted instrument is placed with the axis of azimuth vertical, the observer may proceed through all the angles of his station as fast as he can observe and read them off. They will, however, be affected by the errors of graduation; but it has been shown how these may be diminished; namely, where there are three readings, by reversion; and where there are only two, in cases where extreme accuracy is required, by turning again and again different parts of the circle towards the same object. And although a fresh adjustment of the vertical axis thereby becomes necessary at every step, this is an expedient attended with far less trouble and loss of time than can be brought about by repetition and its requisite accompaniments.

Were it urged in favour of R, that by repetition, the errors of division being almost annihilated, it becomes more fit for ascertaining the position of a star in the heavens; because, in observing one with A, the readings always take place on the same divisions, and consequently the errors of those divisions are charged upon the place of the star: this I should readily grant, were the two instruments equal in other respects. But I have to observe that, in the present state of practical astronomy, neither one instrument nor the other is at all fit for such a purpose. It need hardly be remarked, that the resistance and dragging of the centre-work, which I explained when I endeavoured to trace to its sources the cause of fixed error, will produce the same injurious effect on terrestrial measurement as they do in astronomical observations.

As a transit instrument I believe R was never thought of:

indeed, the imperfect manner in which its adjustment is performed, together with its weak frame, renders it altogether unfit for that purpose. A is a transit, and as perfect as the telescope which it bears will allow. This instrument, in the observatory, is certainly the most important of all, and as a portable one, the best, and attended with the least trouble for keeping the rate of a chronometer. Add to this, that by taking the difference of right ascension between the moon and stars, it affords perhaps the very best means of ascertaining the difference of longitude between distant places*.

For observing equal altitudes, both instruments are very good: and I would only give the preference to A as being more likely to preserve the position of the telescope unvaried, by keeping in adjustments better during the requisite length of time.

For taking altitudes at a distance from the meridian for finding the time, as much may be said in favour of A: yet for this purpose, upon the whole, R seems to have the advantage; because these variable altitudes may be taken by repetition, and the errors of division, to which A is liable, almost done away. Ready means of placing an instrument in the plane of the meridian are of the utmost importance to the travelling astronomer, and that by corresponding azimuths is perhaps the best. A is not only peculiarly adapted to this method, but also furnishes the best means of doing the same thing by all the other ways. When R has a good azimuth circle, its plane may be brought into the meridian by the same means, at least near enough for its own purpose, for it cannot with propriety be regarded as a meridian instrument at all.

Respecting the dimensions of the two circles under comparison, I have, considering every thing, thought that 18 inches should be the greatest diameter for R: this admits of a two feet telescope, and gives sufficient room for the screws for clamping and slow motion to pass each other, and to be conveniently handled. The pressure of heavy telescopes upon the centre-work is certainly detrimental, however desirable good ones may be; and were it not for want of finger room in a 12 inch one, notwithstanding the diminutive telescope which it bears, I would give the preference to the latter dimension. The instrument A may safely be extended to a diameter of two feet, beyond which it could not justly be called portable; but besides this, there are other reasons for not carrying it much further than this limit: and I admire the courage more than the sound judgement of those who have constructed very large instruments on this principle. With regard to the other limit, I would remark that as to instruments smaller than one one foot diameter, however useful they may be for surveying of land and other inferior purposes, I should consider them for astronomy as little better than playthings.

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* Many writers have given an erroneous rule for this purpose: see Phil. Mag. vol. xv. p. 97.

As to its form and general appearance, R is, of all the instruments subservient to astronomy and geodesy, the most uncouth and unsightly. The whole of the effective parts are placed on one side of its single supporting pillar; and on the other a weight, almost equal to the instrument, is placed for the purpose of keeping it in equilibrio. But ugliness is not the worst thing that attends this unavoidable combination; for it renders the instrument top-heavy, tottering, and weak. In these respects A is certainly very much superior. The whole of its fabric is regular and self-balanced; the upper circle, being supported like a transit on two columns, is thus rendered firm and steady. Respecting sightliness, I think the man of taste would, in the different forms it has appeared under, pronounce it agreeable, I dare not say beautiful; and here I may be allowed to remark, that the art of instrument making, as a matter of taste, is far behind many others. In this country, indeed, at the beginning of the art, instruments were adorned with the flourishes of the engraver, chaser, and carver (now long out of fashion): but these are not the beauties which I mean; those of uniformity of figure, and just proportions are alone what I have in view; and I cannot for a moment think that these are at all inconsistent with either strength or accuracy.

Through the whole of this paper every reader will have seen that I am an advocate for A, and I have made no endeavour to conceal it; yet, if I have said more for it than it deserves, or given to R less than its due, it is a thing I am quite unconscious of. Having now finished what I had to say by way of comparison, a concluding remark or two only remain to be added. One of our artists, through a course of twenty-five years, has made the repeating circle under various forms; some of them have repeated horizontal angles, others vertical angles, and more of them have done both; which last have obtained the name of repeating theodolites. All these are of a firmer fabric than that treated of in this communication; yet after having, as he fancies, gone through all the changes of repeating instruments, he owns that he has never satisfied himself in a single instance. I am informed that some of our instrument-makers are at this time endeavouring to improve the repeating circle; but I would submit it to their serious consideration, whether their time and talents might not be better employed in perfecting the art of graduation, and in the construction of instruments of better promise. As it was the rudeness and inaccuracy of dividing which brought this instrument into existence, one would think that as the art becomes cultivated, it will fall into disuse. The art in this country is certainly sufficiently advanced to set repeating instruments aside: and, if I am rightly informed, several foreign artists are at this time pursuing the course of its improvement, in which for many years they had been impeded by circumstances which science could not control. It is therefore my opinion, that as the division of instruments becomes generally improved, so will the repeating circle hasten to its dissolution; and perhaps, on account of the great services, which, in its time, it has rendered to astronomy and geodesy, some future age may be induced to chaunt its *requiem.*

XXI. *Report of a Committee of the House of Commons on Steam-Boats; being the Fifth Report on the Roads from London to Holyhead, &c.*

[We have much pleasure in giving insertion to this Report, so highly creditable to the Committee from which it has proceeded, both on account of the valuable matter embodied in it, and of the sound principles of *non*-legislation which it contains: and are desirous at the same time to express our great satisfaction at the candour and ability with which it is drawn up, and the impartial justice which the Committee have done to the *general* talent and enterprise of the whole country, without any appearance of an intention, which has perhaps formerly been suspected, to eulogize and serve a particular party.—EDITORS.]

YOUR Committee, since they presented to the House their Second Report, have prosecuted the inquiry, then only just begun, into the important subject of steam-boats.

The first instance of applying steam to vessels, is that which occurred in 1736, when Mr. Hull obtained letters patent for the construction of a steam-boat for towing vessels in and out of port[*]. The application of paddle wheels, now so generally adopted, appears to have been originally suggested by this patent. Mr. Hull proposed to employ the atmospheric engine of Newcomen, which, by means of a crank communicating with the working beam, imparted a rotary action to the wheels and paddles which were placed at the bow of the vessel. Next in succession were the experiments of the Duke of Bridgewater, to use steam-boats for towing barges on canals; and then came those of Mr. Miller, of Dalswinton in the county of Dumfries, in a double vessel with the wheel in the middle[†]. But after Mr. Hull, the Marquis de Jouffroy unquestionably holds the most distinguished rank in the list of practical engineers, who have added to the value of the invention. In 1781, he constructed a steam-boat at Lyons of 140 feet in length; and with this he made several successful experiments on the River Soane. In 1795, Lord Stanhope constructed a boat to be moved by steam. In 1801 Mr. Symington tried a boat that was propelled by steam on the Forth and Clyde navigation. Still no practical uses resulted from any of these attempts. It was not till the year 1807, when the Americans began to use steam-boats on their rivers, that their safety and utility were first proved. But the whole merit of constructing these boats is due to natives of Great Britain; Mr. Henry Bell, of Glasgow, gave the first model of them to Mr. Fulton, and went over to America to assist him in establishing them; and Mr. Fulton got the engines he used in his first steam-boat on the Hudson river from Messrs. Boulton and Watt*. Steam navigation seems to have made great progress from this time in America. It appears from the Report of the Select Committee of the Session of 1817†, on this subject, that there were then seventeen large steam-boats in constant employment on the American rivers, besides ferry-boats‡: a list of steam-boats has been published by Mr. Robinson, that shows that on the Mississippi alone, the tonnage of those in work at present, amounts to 7,259 tons, and of those building to 5,995 tons. There are now, in all, about 300 steam-boats in use in America.

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[*] See App. p. 2. of Partington on the Steam-engine.

[†] Ib. p. 57.

Mr. Bell continued to turn his talents to the improving of steam apparatus, and its application in various manufactures about Glasgow; and in 1811 constructed the Comet steam-boat, of twenty-five tons, with an engine of four-horse power, to navigate the Clyde between Glasgow and the Helensburgh Baths, established by him on an extensive scale. The success of this experiment led to the constructing of several steam-boats, by other persons, of larger dimensions and with greater steaming power: these having superseded Mr. Bell's small boat in the Clyde, it was enlarged, and established as a regular packet between Glasgow and the western end of the Caledonian canal at Fort William, by way of the Crinan canal in Argyleshire. Mr. Bell about the same time constructed the Stirling Castle steam-boat, and employed her on the river Forth, between Leith and Stirling; he afterwards took her to Inverness, where she has been for two years plying between that town and Fort Augustus, going seven miles by the Caledonian canal, and twenty-three miles along Loch Ness. Many other boats were successfully established about this time on the Forth and Clyde, and several on the rivers Tay, Thames, Mersey and Humber, and between Southampton and the Isle of Wight; but it was not till the year 1818 that a steam-boat was made use of to perform regular voyages at sea. In this year the Rob Roy, of ninety tons, built by Mr. Denny of Dumbarton, and with an engine of thirty-horse power made by Mr. Napier of Glasgow, plied regularly between Greenock and Belfast, and proved the practicability of extending the use of the steam-engine to sea navigation. In the year 1819, the Talbot, of 150 tons, built by Messrs. Wood, with two thirty-horse engines made by Mr. Napier, plied daily between Holyhead and Dublin, throughout the whole summer and autumn, and successfully encountered many severe gales. In the year 1820, the Ivanhoe, of 170 tons, built by Mr. J. Scott, with two thirty-horse engines made by Mr. Napier, was established on the same station; and in 1821, the Postmasters General introduced steam-boats at Holyhead and Dover for the conveyance of the mails. During these three last years, the Belfast, Robert Bruce, Waterloo, Eclipse, Superb, Majestic, and Cambria were constructed, of large tonnage, and with engines of great power, for conveying passengers between Greenock and Belfast and Liverpool: between Liverpool and Dublin; and between Liverpool and Bagilt in Flintshire. All these vessels, except the Cambria and Belfast, were constructed in the Clyde. In the year 1821, the City of Edinburgh and Mountaineer were established to go between London and Leith; and, in the present year, there have already been fitted for sea the St. Patrick and St. George, at Liverpool; the James Watt, for the Leith and London station; the Swift, to go between Brighton and Dieppe; the Sovereign and Union, between Dover and Calais; and the Lord Melville, to go regularly between London Bridge and Calais *; twelve more are in hand, and will be completed this summer. Ferry-boats propelled by steam, sufficiently commodious to carry carriages, horses and cattle, have been established with great public utility on the Tay between Dundee and Fifeshire; at the Queen's Ferry, in Scotland; on the Severn, the Mersey and the Humber, and at other ferries.

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* See Mr. Watt's Letter, p. 210 of the Evidence annexed to the Report.

† See Evidence of Mr. Seth Hunt. ‡ See Partington, p. 67.

In the Appendix there is a list which has been made by Mr. Field, of all the steam-boats which have been built in the United Kingdom, showing their tonnage† and the power of their

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* See App. p. 198 for the tonnage and power of all these vessels, and for the names of the builders and engine-makers.

† The tonnage of steam-vessels in this Report and in the table in the Appendix, No. 1, is stated according to the customary method of calculating it in other vessels.—The way of calculating the tonnage of steam-boats is specially regulated by 59 Geo. III. c. 5.

engines;

engines; the names of the builders and of the engine-makers; the date of their being launched, and also the station where they ply; from this list it appears, that in the space of a very few years the public have been accommodated, in all directions, with this safe, rapid and œconomical mode of conveyance.

The experience of what steam-boats have performed, is fully sufficient to place beyond all doubt their safety even in the most tempestuous weather. The Rob Roy plied two winters between Greenock and Belfast, and last winter between Dover and Calais; the Eclipse plied the whole of last winter between Glasgow and Belfast, and the Cambria between Liverpool and Bagilt; a steam-boat has plied regularly, through all seasons, between New York, the Havannah and New Orleans; all the other steam-boats which have been used at sea have been exposed to numerous severe gales. But the trial which the Holyhead steam-boats went through during the last tempestuous winter, from the nature of the service requiring them to go to sea at a fixed hour every day, proves that steam-boats, when properly constructed, are able to go to sea when sailing vessels could not, and that in some respects they possess, in very bad weather, advantages over sailing vessels. The following extracts from the evidence of Captain Rogers are quite conclusive as to the power and safety of steam-boats at sea.

" Q. Have you had full trial of the steam-packets, with respect to gales of wind?—A. Yes, in every way. I crossed in the Meteor, on the 5th of February, in the heaviest sea I have seen during eight years I have been on the station.—Q. Have the steam-packets sailed regularly during the whole winter? —A. Except a very few days; I have seen them go several times when sailing packets could not.—Q. Have you found that the steam-packets built under the inspection of the Navy Board are as safe as any vessel you ever navigated?—A. Certainly.—Q. Is there any danger, in your opinion, to be apprehended from them as steam-vessels?—A. No.—Q. Are you of opinion, that in the event of the engine failing, with the assistance of sails and the anchor you may keep a packet in perfect safety?—A. There is no doubt of it; by putting two cables together, which she has on board, she would ride out any gale in the channel as easy as a glove.—Q. Are the Committee to understand your opinion to be, that in any weather, however severe, the steam-boats will stand that weather as well as any sail-boat?—A. Yes, in any wind; the more wind the better for the steam-boats; that is where they show their superiority.—Q. In the heaviest gale that could blow, you would rather be in a steam-packet than a sailing-packet?—A. Yes;

that is, in a vessel of our construction.—Q. Have you found, in blowing weather, that the vessel works at all, either inside or out?—A. No, not at all; I do not see it in the least, not a single thing, she is as solid as a wall.—Q. Was the last winter a worse winter than usual?—A. I have heard it said that it has been the worst winter for fifty years; Lloyd's have paid more this winter than ever they did.—Q. Can you carry one boat on each quarter?—A. We can carry two, but all that is lumber; we never think of being drowned or burnt now." In another place Captain Rogers says, "I never read a novel before I was on board a steam-packet, and I go down now frequently and read for an hour or two."

The following extracts from the answers of the other Holyhead captains, corroborate the evidence of Captain Rogers. Captain Goddard says, "These vessels by their performances through the past winter, have exceeded the most sanguine expectations; and certainly have made passages across the Channel, when the sailing-packets would have found extreme difficulty to have accomplished them; and in so short a period of time as places their performance beyond the necessity of comparison to establish their great superiority."—Captain Skinner says, "I am of opinion, a steam-packet of about 180 tons burthen, form similar to that of the Meteor, with a little finer entrance and strength of building, with masts and sails the same, would be the best; a vessel of that description would be able to make a voyage, when it was fit weather for any other vessel to put to sea."—Captain Davies says, "the two vessels on this station have answered wonderfully well."

The testimony of the Holyhead commanders is not only extremely important, in consequence of the experience they have had of the performance of the steam-packets during the last winter, but also because it is to be recollected, that even after the Talbot and Ivanhoe had been on the station, it was their opinion that no vessel could perform the winter service with safety but sailing-cutters, such as the old Holyhead packets. This opinion it was natural they should entertain, knowing so well as they did. the heavy seas and desperate gales which frequently prevail for weeks together in the Irish Channel. But the trial of last winter having now brought them to acknowledge a change of opinion, this circumstance does every thing that by possibility could be wanting to establish, upon the best authority, the safety and superiority of steam-boats for this service.

Notwithstanding the great number of steam-boats which have been constantly in use, in different parts of the kingdom, during the last ten years, very few accidents have occurred, and these few were chiefly owing to the novelty of the experiment: so many precautions are now taken that there is no reason to apprehend the recurrence of any serious accidents. The general use of low pressure boilers made of wrought iron or copper has removed the possibility of accidents from their bursting. If one of these boilers gives way, the materials do not fly, but are rent asunder. This part of the subject was very fully investigated by the Select Committee on Steam Navigation in 1817; and the evidence given before that Committee contains every thing that is necessary to remove all apprehensions of danger from the bursting of low pressure boilers. In respect to the furnaces, they are so constructed that there is no danger from fire, because there is water all round them. Mr. James Brown says, "I hardly think it possible that fire can take place, because the furnaces are completely surrounded with five inches of water round every part." The coals are kept in iron cases so as to prevent all communication with the fires; and if, in addition to these precautions, vessels are supplied with extinguishing fire-engines, there is no danger of accidents from fire. It has been suggested, that steam-vessels are not provided with a sufficient number of boats, and that an Act of Parliament should be passed to require every vessel to carry a certain number, according to her tonnage: but your Committee, after the fullest consideration, are strongly of opinion that the policy of avoiding to do any thing that could by possibility check the spirit of improvement which now is so prevalent, and which promises such great advantages to the public, is that which ought to be followed. It is to be remembered, that the expense of fitting out steam-vessels is very heavy, and that proper experiments of new inventions cannot be made but with the risk of incurring considerable loss; and as nothing would check the zeal of those who are disposed to make such experiments so certainly as the meddling of officers exercising the powers of a regulating Act of Parliament, nothing could be more baneful than the interference of the Legislature with this new branch of science. The Ballast Office of Dublin brought a bill before the House of Commons last year, for the purpose of appointing inspectors over the Liverpool and Dublin packets; but the chief secretary of Ireland, Mr. Grant, very judiciously put a stop to its progress. Individual security in steam-boats will always be sufficiently provided for, by the interest of the proprietors constantly contributing to lead them to do all those things which will best obtain the custom of the public. Competition in this case, as in all others, will more effectually establish those precautions which are right to be taken, than the best devised regulations

gulations of an Act of Parliament. But at the same time that your Committee decline to recommend any legislative control, they are decidedly of opinion that the owners of steam-vessels who omit to provide a sufficient number of boats, to secure the safety of their passengers, in case of any sudden accident, are guilty of great neglect, and not deserving of the countenance and support of the public. Besides this precaution, in respect to boats, there ought to be on board every steam-boat, for the perusal of the passengers, a certificate of some experienced engineer, to testify the strength of the boilers; the sufficiency of the valves; the safety of the furnaces, and the general good condition of the vessel and machinery.

The speed and regularity with which steam-boats perform their voyages, are the next point worthy of being brought under the notice of the House.

The average length of the voyages of the Holyhead packets, from the 1st of June 1821 to the 1st of June 1822, has been about seven hours and a half; the average of the sailing-packets was fifteen hours. Captain Percy, who commands the Hero London and Margate packet, says, "We generally make the passage in seven hours and a half, the distance being eighty-four miles." Mr. James Brown says, "the Edinburgh Castle has gone from London to Leith in fifty-eight hours, a distance of 450 miles; but that the James Watt is a faster vessel, her speed being ten miles an hour through still water, independent of wind and tide." Mr. Traill states, that the Majestic has performed the voyage from Greenock to Liverpool, a distance of 240 miles, in twenty-two hours; and that the St. Patrick came from Dublin to Liverpool, 130 miles, in thirteen hours and a half, against a stiff breeze from the east. The Lord Melville goes from London Bridge to Calais in eleven or twelve hours. This great speed with which the voyages are made by steam-boats, adds considerably to their superiority over other vessels in point of safety; for in the same degree that the time occupied in performing a voyage is diminished, so is the risk of danger also to which passengers are exposed.

It is now evident that the failure of all the early attempts to apply steam to sea boats, was owing to their being built too square; to their want of strength, and to the want of a sufficient quantity of steaming power. According as boats have been built with a form planned on better sailing principles, with greater strength of timber, and with engines of increased power, the progress of their success has exactly corresponded with these improvements.

On referring to the list of steam-boats in the Appendix, the Talbot and Ivanhoe on the Holyhead station, and the Belfast, Eclipse, Superb, and Majestic on the Greenock and Liverpool passage, will be found to be the first strong and powerful boats which were built, and they were the first that completely succeeded. The strength and power of the Holyhead packets are clearly the cause of their success; and the still greater power which has been given to each of the new boats, the St. Patrick and St. George, lately built at Liverpool, namely, of two fifty-five-horse engines, promises to make them superior to any of their predecessors.

Your Committee having thus briefly given a general description of the rise and progress of steam navigation, will now proceed to make such observations, as the information they have obtained seems to justify, upon the more scientific part of the subject: and in doing this, they will divide it under four heads;—1st. The form of the vessel;—2d. Her strength;—3d. The machinery;—4th. Sails.

1. *Form of Vessel.*

Captain Rogers says, "In building a steam-boat she ought to have a fine entrance, and her bow to flear off, not to shove any water before her; she should have a good line of bearing, and her transom pretty square, and not too high: the transom being square and low, and fine under, so as to give her a right line of bearing, will stop her pitching and rolling, and make her easy on the sea, and add to her speed."—Captain Townley, who has been commanding steam-boats, since 1819, between Dublin and Liverpool, says, "As to form, a steam-vessel should have an extreme fine entrance below, rise well forward, and flam off, so as to let her fall easy into the sea, and throw it off when steaming head to wind; she should have but little rise of floor, so as to be pretty flat under the engines, and run off as clean as possible abaft: I approve of giving them a good deal of rake forward."—Captain J. Hamilton, of the Arrow Post Office Dover packet, recommends for wet harbours "a vessel with a rising floor about three inches hollow, to prevent her rolling; fair and easy curved water-lines; the stem to rake well, which makes her easy, going head to sea; the stern post to stand square to the keel, and to draw from seven feet nine inches to eight feet water."—Mr. John Scott, ship-builder at Greenock, says, "I have continued to make the fore body of my vessels very fine, with a good entry, which I have always found made the vessel sail faster, and easier impelled."—Messrs. Maudslay and Field say, "The form of a steam-vessel under water should be that of the fastest schooner, bold at the bows, the whole vessel rising but little out of the water; the sponcings, or projecting work on the sides, added to the proper body of the vessel, and rising from the water-line at an angle no where exceeding forty degrees from the perpendicular of the side; the bulwarks, wheel-cases, and all the exterior of the vessel, smooth and free from projections that would hold the wind."—Messrs. J. and C. Wood say, "The vessel should be formed with a fine entrance and run; sharp raking bow both below and above; a broad transom not too high placed; a good rise in the floor, limited by the draught of water and the occasion of taking the ground."

II. *Strength of the Vessel.*

The regularity, speed and safety with which the Holyhead steam-boats crossed the Irish Channel, throughout the whole of last winter, are the best evidence of the vast importance of great strength in the construction of this description of vessels. Captain Rogers says, that he would rather be in a steam-boat, in the heaviest gale that could blow, than in a sailing packet, if constructed like the Holyhead steam-boats; and it is evident, from his whole testimony, that the great confidence he places in them is on account of their prodigious strength. He says, "Their strength is owing to their being filled up solid to the floor head; to the timbers being put together and diagonally fastened on Sir Robert Seppings's plan; to their being caulked inside and out, having no tree nails, but bolted, and copper fastened; the bolts being driven on a ring clinched at both ends."

Sir Robert Seppings, in his answers to the printed queries of the Committee respecting the proper strength of a steam-boat, says, "In point of strength, I consider that the principle introduced into the Sovereign and Meteor (Holyhead packets) should be generally adopted in all steam-vessels; and in fact in all other vessels, but particularly in those of the packet class; as it gives safety in the event of the loss of the keel, and also a proportion of the plank of the bottom; either of which would be the destruction of a vessel constructed on the common principle." Sir Robert Seppings having delivered, with his answers to the printed queries, a description of his mode of building ships, accompanied with drawings to explain it, your Committee have inserted them in the Appendix to this Report, in order to give as much publicity as possible to his valuable invention.—Messrs. Maudslay and Field say, "The straining of a vessel at sea has frequently broken some parts of the machinery; but in a vessel well adapted for the open sea, her great strength should be a security against accident from this cause; the vessel should be exceedingly strong."—Mr. J. Cook, of Glasgow, recommends that a steam-vessel of 180 tons should

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be built with a scantling for a sailing-vessel of twice that tonnage.—Mr. Roger Fisher says, "There has been, in my opinion, a great improvement made in the strength of steam-vessels built here (Liverpool) lately; that is, by carrying the frame timbers up so as to form the projection of the sides, and then regularly planked up solid as any other part of the vessel, by which means they are much safer."—Mr. Brunel, when asked whether he would recommend a steam-boat to be built much stronger than usual for sailing-vessels? gave the Committee to understand, that great weight would be injurious, by lessening the buoyancy of the vessel; but Captain Rogers's evidence corroborates the opinions of the other witnesses, and seems to show that this inconvenience does not follow:—"Q. Have you found those two vessels (the Holyhead steam-boats) equally buoyant with any other you have sailed in?—A. Yes.—Q. Has the great weight of their timbers, and other materials, diminished their speed?—A. No; I think it rather gives them speed against a sea.—Q. Then the inconvenience anticipated from the mode of constructing these vessels has not taken place?—A. No; it has not."—Mr. George Henry Freeling says, "I have attempted to gain some information about every steam-vessel which has been built; and I am convinced these vessels (the Holyhead packets) will do what no other vessels can do,—they will go to sea in weather when nothing else can; I attribute that not only to the machinery, but to the great weight of the hull; a lighter vessel in a heavy sea would be checked; but these vessels have from their weight a momentum so great that it carries them on when a lighter vessel would be checked; the weight acts as a fly-wheel."—"I have the authority of Mr. Lang of the Navy Board for stating that, with the exception of the Discovery ships, there are no vessels so strong."

III. *Machinery.*

The steam-engine, employed on board ships, is as yet a much less perfect machine than when it is used on land; the height of the cylinder is nearly one half less; the power is thereby cramped by short strokes, which are incalculably bad. In this way there is a great loss of power, as the *vis inertiæ* is to be overcome on every stroke; more frequent alternations are necessary of the beam, the piston and the valves, which occasion more wear, and more friction than where the cylinders are made longer. There is also a considerable loss of power in converting the alternate motion of the piston into the rotary motion of the paddles.

The engines, as now used at sea, want some contrivance to enable them to bear with the irregularity of the pressure on them *; sometimes there is a vast excess, sometimes a deficiency, and sometimes a total absence of the resisting medium. "These irregularities throw the ungoverned and almost irresistible power of the steam-engine into those convulsive starts that meet no other controlling check but the arm of the crank, from which it recoils with increased energy to the opposite side, occasioning thereby those destructive shocks, those alternate strains and wrenchings, which a frequent recurrence must render fatal to the cranks and to the shafts, besides other parts of the machinery." To remedy these defects by such combinations of machinery as may enable the engine to adapt and accommodate itself spontaneously to all the exigencies incident to its peculiar service, is one of the chief objects which should attract the attention of engineers.

The great size of the boilers, as now made, is very disadvantageous. They occupy a very inconvenient portion of the space within a vessel.

The method of fixing the paddles is a very defective part of the machinery: the oblique action of them in entering and departing from the water, produces that tremulous jarring which serves to loosen the seams and the bolting of the knees and beams of the vessel; it also occasions a very great loss of the steaming power.

In respect to the degree of strength proper to be given to the machinery, almost all the engineers, who have been examined, concur in the opinion that it ought to be very considerable. Messrs. Wood say, "All the connecting machinery should be twice the strength for ordinary work on shore." Mr. Donkin says, that every part of the engine should be made at least of three times the strength, which, by estimation, would be required for any force to which it might be exposed. "Accidents," he observes, "are most likely to happen at a time when the suspension of the power of the engine would be most fatal."

Wrought-iron is strongly recommended to be used in place of cast-iron; and though some of the witnesses have expressed doubts of the practicability of making large shafts of wrought-iron, Mr. Donkin does not hesitate to say, that "they can always be got quite perfect, if a sufficient price is given for them."

As so much of the safety of the vessel depends upon the workmanship of the materials, they should be proved before they are used, by a proper proving engine for trying their strength, as well by a force acting in a twisting direction, as by a strain in the direction of their length.

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* Mr. Brunel's evidence, pp. 175, 176.

When Mr. James Brown was asked by the Committee, "Is an engine liable to that extraordinary pressure upon it, under particular circumstances, that it is better to have some part of it that would give way?" replied, "I should think so."—Mr. G. H. Freeling says, "There must be some part of the engine left to give way in case of any emergency, which is better than destroying the cylinder."—But Mr. Timothy Bramah, Mr. Donkin and Mr. Field, say, that "the engine should be made so strong, that it may be brought to rest without the fracture of any of its parts, in case it met with a resistance that would require its ultimate power. They mention instances that have come under their own personal observation, of engines having, in this way, stopped with no other effect than that of the steam forcing open the safety valve and going off.

It may be collected from the evidence, that the greater part of the breakages which have occurred of different parts of the machinery in steam-boats, has been owing to the negligence of the engine-keepers: starting the engine without clearing off the water which is formed on the top of the piston, from condensed steam. is one cause of fractures; other accidents have arisen from suffering the bearings upon which the shafts work, and the links connecting the piston with the beam, to get loose; and in some cases from making them so tight, that the bearings heat; and also from not attending carefully to the steam-valve when the vessel is exposed to a heavy sea.—Mr. Watt says, "With the experience now obtained, we make no doubt but that we shall be able to construct machinery less liable to accident; but much must always depend upon the vigilance and experience of the men who work the engines.—Mr. James Brown being asked what were the causes of accidents to the machinery, replied, "They depended more on the engine-keepers than any thing else."

Mr. Donkin says, "I have reason to believe that some of the steam-boat companies have suffered severely from a want of regular professional inspection;" and being asked, "Do you conceive that the injury to engines from neglect is greater than the injury arising from the actual working of them?" replied, "Yes, I do;" and being further asked, "Has that been a constant defect in the management of steam-boats up to this period?" replied, "Yes, I conceive so."

All the evidence is so decidedly in favour of making boilers of copper, that it is necessary only generally to refer to it. Messrs. Fenton and Murray, of Leeds, say, "The boiler ought to be what we call a combined boiler, viz. three distinct boilers put together to form one boiler, with the fire passing three times through each, and so constructed as to be taken up and down a hatchway without pulling up or destroying the decks."

Mr. Donkin and Mr. T. Bramah are of opinion that all boilers are now made too large, and that the same quantity of steaming power might be obtained with a smaller body of water, if the surface of the boiler exposed to the fire was sufficiently large.

All the witnesses agree in opinion as to the necessity of keeping the machinery as low as possible in the vessel: Mr. Watt says, "This will diminish the top weight, make the vessel more steady at sea, improve the action of the machinery, and add to the safety of the vessel."—Messrs. Maudslay and Field say, "The best arrangement of the machinery, and in which engineers are most agreed, is to place the boiler or boilers a few feet abaft the centre of buoyancy of the vessel; the two engines on each side, a few feet forward of this point; and the coals on the centre of buoyancy: this arrangement brings the fuel, which is constantly variable, on a point that will not affect the trim of the vessel; it also brings the wheelshaft, which is at the foremost end of the engine, to its best position as regards the length of the vessel, viz. at about one-third from the head: the weight of the boiler, engine and coal, is thus spread pretty equally over the space allotted for them, and partial and intense weight on any one part is thus avoided."

Several of the witnesses having mentioned the injurious effects of sea-water upon the boilers, your Committee examined Mr. Michael Faraday, who acts as chemical assistant to Mr. Brande at the Royal Institution, concerning the chemical properties of sea-water. Mr. Faraday's evidence explains the temperature and degree of saturation of the water in the boilers at which various salts are deposited, and by what process the metals of which the boilers are made, are injured. It appears that the greatest care is requisite on the part of the engine-keepers to prevent the water in the boilers from being so much saturated as to occasion the deposition of the salts. When this takes place, these salts corrode the metal, and destroy a portion of it, and form crusts over the internal surface of the boilers, which having bad conducting powers as regards heat, diminish the quantity of the steam, and cause the fire to burn the boilers and the flues.

Mr. Faraday explains, that the injury done to iron boilers by the deposition of salts, is much greater than the injury which is done to copper boilers;—an additional and a very strong reason for using the latter.

Messrs. Maudslay and Field state, that the fire-places and boilers are frequently burned and injured from the incrustations made by deposited salts in the boilers, through neglect to change the water and clean the boilers; and Mr. Donkin says he has known great inconvenience from the same cause. In one instance, going to Margate, one out of three boilers in the vessel produced very little steam, in consequence of the incrustations on the bottom, a circumstance that was discovered by its requiring very little water to be introduced into it. Mr. Donkin further says, that he knows only of two methods by which the deposition of salt can be prevented. "In the Regent steam-boat, they employed a method very successfully, that of pumping hot water through the boiler, and allowing a certain quantity constantly to be discharged from it into the sea; by these means the water was always kept in a sufficiently diluted state, so as to prevent its becoming saturated with salt, and consequently none could be deposited. No other inconvenient effect was produced than a greater consumption of fuel. The other mode is the common and ordinary one of taking out the whole of the water when the vessel arrives at the place of destination, and if there is any deposit of salt, taking that out also."

In consequence of the injury which sea-water does to iron, Mr. Cooke recommends that the air-pump, buckets, rods and valves, should be made of copper or brass.

It is necessary that great care should be taken in selecting coals to be used in steam-boats; they ought to burn free and become complete white ashes, without caking on the fire bars. Mr. Brown says, "that in the first passage he went in the City of Edinburgh to Leith, they were obliged to clear the fire every four hours: but having got a better description of coals at Leith, called Halbeath Main, they went, in coming back, sixteen hours without clearing the grates."—Mr. Donkin says, "I think the coals ought to be particularly attended to; first, the kind of coals, and secondly, to avoid taking small coals, so long as the common fire-places are used; small coals occasion great waste; and all coals employed for steam-boats ought to be screened*." There is another reason why attention should be paid to the selecting and managing of the coals, arising from their tendency to fire spontaneously, if put together in large quantities in a damp state, and then exposed to the heat of a steam-boat.

It will be seen by the evidence, that the consumption of fuel differs very much, according to the various plans on which the engines are constructed, even where the steaming power is the same.

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* See Evidence of Mr. Donkin, p. 172, and of Mr. Faraday, p. 190.

In adapting the quantity of steaming power to vessels, the object, as yet, seems to have been to obtain a great degree of speed in smooth water. But this principle, in respect to sea vessels, is clearly an erroneous one; for the proper object is not so much having great speed through smooth water, as a certain progress, even at a very moderate rate, against a head sea in a heavy gale of wind.

The quantity of power sufficient to accomplish this is that quantity which should be applied to every sea boat, if her size and draught of water will admit of it. Mr. Donkin says, "From the observations I have made in most vessels, I have found them to have too little power for the size of the vessels:" he recommends that the engines should be made considerably larger than necessary for giving the vessel the required velocity in still water: he says, "There is no other disadvantage from increasing the power of an engine than the room it requires and the expense; but to counterbalance these, there is an advantage in not being obliged, at all times, to work the engine up to the extent of its full power; less fuel would be consumed, and the engine would be less likely to go out of order."

Mr. T. Bramah says, "You cannot have too much power; indeed it is always of advantage to have as much power as can be obtained."—Messrs. Maudslay and Field say, "With regard to the quantity of power proper to put into a sea-vessel, the only limit should be the weight of the engine and fuel the vessel will carry and contain; no vessel ever had too much power, even in still water, much less when contending against a heavy head wind." "Two engines," they go on to say, "of half the power each, are more manageable, and possess many advantages over one of the whole power; they produce a perfectly uniform rotation in the wheels, and are not subject, like single engines, to be stopped on the centre in heavy seas; and in case of injury to one engine, the other is available."

It appears from Mr. Brown's evidence, that two fifty-horse engines will weigh from 20 to 25 tons more than two forty-horse engines; the weight of the latter, with coal and water complete, being 100 tons. The additional expense would be about 1000*l.*; the expense of two forty-horse engines being about 6000*l.* According therefore to the opinions already stated, when a vessel will contain two fifty-horse engines, it will be decidedly better to have them of this power than two of forty-horse power.

Notwithstanding the great and rapid progress which steam-navigation has made, it is still considered by the ablest engineers to be only in its infancy: experience suggests, in every new vessel and engine, some improvement to be made, or some defects to be removed. The numerous companies that have been formed in so many parts of the United Kingdom, have established an extent of competition which necessarily excites all the science of the country, to seize on every opportunity of making every thing better than it has been made before; and as the confidence of the public in steam-boats leads to the general use of them, there is that fair reward for enterprising undertakings which will effectually sustain the general spirit which prevails amongst all the most scientific engineers, seamen and ship-builders, to invent further improvements.

It appears from the evidence, that attempts are now making by very ingenious individuals to remove some of those defects, which have been described to belong to the engines now in use.

Mr. Brunel is engaged on a plan for making the engine more compact and more simple, and at the same time stronger; and to enable it, by certain mechanical combinations, to adapt and accommodate itself to all the exigencies and to all the perturbations incident to its peculiar services.

Mr. Galloway and Mr. Perkins feel confident that high pressure boilers may be so contrived as to be used with the greatest advantage. Mr. Perkins, in his answers to the circular queries, gives such strong evidence in favour of them, from the actual use of them in 150 American steam-boats, as to go far towards removing the prevailing objections to them.

Mr. Donkin is of opinion that a rotary furnace, on Mr. Brunton's principle, may be applied to steam-vessels. This would be so very valuable an improvement, that your Committee beg the attention of the House to Mr. Donkin's evidence on the subject: "Q. Are you of opinion that this apparatus may be applied to engines on board ships?—A. Yes, I think, with very great advantage; it probably would require a little variation in its construction.—Q. What are the general advantages of this plan?—A. The general advantages are œconomy of fuel and labour, and greater safety to vessels.—Q. In what way do you consider it would contribute to greater safety?—A. Because it prevents the continual operation of feeding the fire by hand, which requires the fire-doors to be opened every five or ten minutes; and the frequent stirring of the fire occasions a great deal of the ignited coals to fall through the grate and upon the floor, whereas in this apparatus the coals are supplied by the machine itself, the fire-doors need not be opened except about twice or three times a day.—Q. Does the fire produced by this apparatus act more regularly or powerfully than the fire supplied in the ordinary way?—A. Yes, it does; small portions of coals are introduced on a revolving fire-place at certain intervals, so that the fire is regularly supplied;

plied; the combustion goes on entirely at the surface, and the incombustible matter, such as the vitrified parts of the coal and the ashes, settle down upon the bars, and protect them from the action of the fire." Your Committee have inserted in the Appendix to this Report the evidence given by Mr. Brunton, concerning these furnaces, before the Select Committee on steam-engines, of the session 1820.

Mr. Oldham, of the Bank of Ireland, has invented a plan of revolving paddles, to avoid the defects of the fixed paddles as now used. He states, that the violent action of the paddles of common wheels, in striking the water in a rough sea, is entirely removed by the use of the revolving paddles, as they enter and rise out of the water with a peculiarly soft and easy motion. The precise merits of this invention will soon be ascertained, as these new paddles are now fitted to the Waterloo packet, which plies regularly between Dublin and Liverpool, and to the Aaron Manby iron steam-boat, lately sent from England to Havre-de-Grace, to be used on the Seine.

Mr. John Gladstone, of Castle Douglas, has invented another plan of paddles. He employs a pair of wheels at each side of the vessel, having two endless chains acting on them, with paddles fixed on these chains; and so far as the plan has been tried, on a very small scale, it has been successful. Several attempts have been made to get rid of the use of external wheels, but hitherto without success.

Mr. Field has invented a flexible metallic piston, which has proved of great utility.

Your Committee have endeavoured to avoid all partiality in the arrangement they have made of the evidence under separate heads, and in their observations upon it; their object has been to bring as concisely as it was practicable, under the notice of the House, all the prominent advantages and defects belonging to the actual state of steam navigation at sea; with the opinions of the most scientific professional men upon the best means of improving it. If the frequent reference to the performances of the Post-office Holyhead packets should give rise to an impression that your Committee consider them superior to other steam-boats, your Committee desire it to be understood that they do not think any comparison can be fairly made between these vessels and any hitherto built by private companies. In the first place, the funds for constructing them, being the public money, admitted of a scale of expense that private companies could not be expected to incur; and, in the next place, those vessels were built for voyages to be commenced at a fixed hour every day in the year, across a sea exposed to strong tides and heavy gales; that is, for a duty entirely different from the duty of common packets. From the description which has been given to your Committee of the new Liverpool steam-boats, the St. Patrick and St. George, the Committee are led to expect that these vessels will prove superior to the Holyhead packets, if they have been built of equal strength, in consequence of the great power of their engines, each being provided with two fifty-five-horse engines.

The merit of first applying steam-engines to sea navigation is certainly due to the skill and enterprise of the engineers and shipbuilders of the Clyde; for it was unquestionably the success of their steam-boats on the Holyhead station, which led the Post-office to establish their boats for keeping up the communication between the two countries. At the same time it is but justice to say, that the public are greatly indebted to the Post-office for having exercised such a sound judgement in directing their vessels to be built of that great strength which has been so often mentioned in this Report; and which, at the same time that it has been the cause of their complete success, has also established a new principle of certainty and security in the system of steam navigation.

IV. *Sails.*

It does not appear to your Committee that there is any probability, at present, of applying sails to steam-boats in any more effectual way than they are now used. Captain Rogers says, "they assist a vessel very much; that they can be used at all times, except within four points of the wind, and that they serve to keep the vessel steady." He recommends a large lug-sail forward, a jib and fore and aft mainsail; to have a square topsail on board, and a gaff topsail aft; with means of setting a topmast, but not to use it unless the engine was out of order. Several plans have been tried for getting the wheels out of geer, and for moving the paddle-boards from the extremity of the wheels towards the centre; and some of them successfully. By these means, a vessel, in case the engine cannot be used, may be sufficiently well managed with the sails, as to carry her safely into port. The evidence of all the other witnesses goes to show, that any attempt to carry canvass beyond a certain moderate quantity, will be attended with a great impediment to the steaming power.

The great importance of a thorough acquaintance with every thing belonging to steam navigation, for securing a certain and rapid conveyance of the public correspondence, where seas are to be crossed, has induced your Committee to take this general view of the whole subject, as a preparatory step to coming to that part of the order of the House, which requires your Committee to examine into the conveyance of His Majesty's mails between Holyhead and Howth.

Your Committee consider the information which has been given by so many distinguished practical engineers and ship-builders, as extremely valuable; and that every praise is due to them, for the readiness and zeal with which they have contributed to render the inquiry of your Committee of general utility. Not only the Post-office, but all private companies engaged in steam-boats, may obtain great assistance from opinions derived from such extensive sources of science and experiment; at the same time that the public will be benefited by those various improvements which they suggest, and which will be the natural consequences of the persevering efforts of the great talents which distinguish these professions in Great Britain.

Your Committee, in expressing their opinion in respect to the proper establishment of steam-boats at Holyhead, concur decidedly with the great majority of the witnesses, who say that not less than four steam-boats ought to be employed on this service. They recommend that the vessels should be built of very strong timbers, put together, filled in, and diagonally fastened, according to Sir Robert Seppings's plan; and that they should be coppered and copper-fastened throughout. They approve very much of the plan of a steam-boat as described by Messrs. Maudslay and Field, who say, "A steam-vessel and engine to encounter a gale that would bring a stout frigate under her double-reefed topsails, or a good cutter under a three-reefed mainsail, should be a vessel of about 200 tons; both vessel and machinery exceedingly strong; her form, under water, that of the fastest schooner; the centre of gravity kept as low as possible; the projecting works on the sides added to the proper body of the vessel: the rigging to strike completely; the chimney formed to cut the wind; with two fifty-horse engines every way proportioned to the strength of the vessel." Your Committee conceive the evidence, which has been given before them, removes all doubts in respect to the practicability of putting engines of this power in a vessel of 200 tons.

Your Committee are of opinion, that every part of an engine for a Holyhead steam-boat should be made of wrought-iron, except where there is no risk of breaking, and should be effectually proved before using it, by a proper proving machine; that the boilers should be made of copper; and that the air-pump, buckets, rods, and valves, should be made of copper or brass.

Your Committee particularly recommend, as indispensably necessary for œconomy as well as safety, according to the opinions of all the witnesses who were examined to this point, that a professional engineer should be employed to reside constantly at Holyhead, to superintend the machinery and inspect the engine-keepers. And also that each vessel should be supplied with an extinguishing fire-engine, and with two large boats in addition to the ordinary ship's boat.

From the great advantages which may be derived from revolving furnaces, your Committee feel anxious that a proper experiment should be tried to ascertain whether they can be used in place of the common fire-places.

[The Report concludes with some suggestions relative to the management and fares of the steam-boats between Holyhead and Dublin, as well as the Custom-house arrangements, docks, roads, and Post-office regulations.]

XXII. *Fossil Bones on the Coast of East Norfolk. By Mr.* RICHARD TAYLOR.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—I BEG to communicate an extract from some geological memoranda made during an excursion a few days ago along the Norfolk coast, from Cromer southward;—my object being chiefly that of pointing out the localities of an extensive stratum of osteological remains.

Throughout the course of the cliffs which form the eastern boundary of this county against the German Ocean, from Happisburgh to the north of Cromer, may be traced, at intervals, along the base of the clay cliffs, a remarkable stratum containing an abundance of fossil wood and the bones of large herbivorous animals mineralized by iron. The thickness of this singular bed does not exceed two feet, and frequently not more than one. It varies in its material, from a red ferruginous sand to an ochreous coarse gravel cemented by iron, and often divided into septa by a coarse ferruginous kind of crystallization, accompanied by thin, flattened, and circular cakes of very hard argillaceous red-coloured stone: others are spherical, from the size of a hazel-nut to that of a hen's egg, and resemble the seed-vessels figured in the first volume of Parkinson's Organic Remains.

The vicinity of the stratum which I shall proceed to describe, is always indicated by the abundance of these stones, which are washed to the base of the cliffs, and, being too hard to be readily injured by attrition, are sometimes accumulated in considerable quantities. Many of the nodules having been split into laminæ by the operations of the air, moisture or frost, are again united by a cement of hard ferruginous sand; and in this state some of the fossils and bones are discovered.

The most numerous organic substances here are those of vegetable origin, in various degrees of preservation and mineralization, from the state of black and rotten peat wood to that of a ponderous iron-stone, somewhat flattened by pressure, as I believe is the case with all fossil wood. When any portion of this stratum is exposed horizontally on the beach, fragments of oak-wood several feet long are often uncovered by the waves. It is probable that the bed containing this wood extends along the coast, below the level of the sea, much more to the south than Happisburgh; for large masses in all stages of preservation are continually thrown up on the beach as low down as Caister, Winterton, and Palling. We should even be correct in stating it to be an extension of the well-known stratum at Watton-cliff and Harwich. The part of the Norfolk coast where it is most conspicuous, is at Overstrand, about three miles south of Cromer. Here some small springs of chalybeate water ooze out of the ferruginous bed before noticed, imparting to the pebbles of the beach and to the waters left by the tide a strong tinge of bright brown or red. Here and there are scattered heavy nodules of radiated pyrites, which are brilliant when broken, and of the colour of brass; and a strong sulphurous smell is emitted, particularly in warm weather. Some pieces of iron thickly incrusted with ferruginous sand and shingle were completely metallic at the core.

A few of the flattened stones have casts and impressions of shells upon their surfaces, particularly some species of Astarte or Venus.

But the most important amongst the organized remains here are the *reliquiæ* of land animals, of which the elephant and the deer are the most conspicuous. A fine grinder of that which bears a close affinity to the East Indian elephant was recently detached by me from this stratum. It is ponderous and discoloured; for it is probable that iron now forms one of its chief component parts and has added much to its weight. The plates which remain are nine in number; the enamel is perfectly white, and the intermediate spaces are of a deep black: the whole length of the triturating surface is about six inches, and when perfect was originally much longer. See Plate II. fig. 2.

Embedded with this was what I conjectured to be the upper part of the skull of an animal equal in size to the elephant.

phant. This was fifteen or eighteen inches in diameter, and upwards of three inches thick, externally black and perfectly smooth. Unfortunately this specimen was not capable of removal, except in fragments, from its having been divided into a number of pieces by the splitting into the kind of septaria and subsequent filling up with ferruginous matter mentioned before. This mass probably weighed fifteen pounds; and I regret that it was so decomposed and distorted, from these circumstances, as to render it doubtful as to the precise original shape.

Fragments of huge bones, more or less worn by attrition, according to the time in which they have been removed from their sites in the cliff, are met with on the beach. Some of them are light and cellular, but for the most part they are very heavy, deriving their weight from the iron with which they are strongly impregnated.

Nearer Cromer, embedded in the same stratum, I met with the upper part of the skull or frontal bone of an animal of the elk kind, having a portion of the horns remaining, but broken off a little above their bases. Its surface is smooth, black and shining; extremely ponderous, the forehead about six inches broad. Fig. 3.

Another skull which much resembles this, accompanied with vertebræ of land animals, I have obtained from a similar stratum of ferruginous gravel a few miles south-west of this spot, a continuation, probably, of that which is exposed in the cliff between Mundesley and Cromer. Fig. 1.

Fossil bones have long ago been noticed in the cliffs of Norfolk as occurring incidentally, for it was not known that they were stratified. Sir Thomas Browne communicates in a letter to Sir William Dugdale, in the year 1659, that the head and bones of a very large fish were discovered near the top of Happisburgh cliff, by the falling down of a part of the soil in which they were embedded.

The fossil grinder of an elephant of the Asiatic species was also found here, in 1805, by Mr. William Smith, and is now with his collection in the British Museum.

It is probable that a more extended osteological examination will lead to the discovery of the mineralized remains of other animals in this situation. At present we have added one to the many authentic instances of the remains of the stag being associated with those of the elephant. We may add a further instance in the neighbourhood of Norwich, where the horns of stags are associated with the opalized teeth of the mastodon or mammoth and the grinders of elephants.

The stratified organic remains in the cliffs of East Norfolk are buried beneath beds of blue clay, earth and sand, from 80 to 100 feet in thickness. Some of the indurated and flattened stones belonging to or immediately in contact with this stratum, contain well-preserved specimens of Astarte, Pecten, Cardium, Terebratula plicata, large Serpula, Trochus, Nucula? and a small pyritous ammonite with foliated septa.

In the clay substratum are boulders of strong dark blue clay, in which the fossils assimilate to those of the clay in the environs of London. Sharks' teeth, Tellina, Cardia, and Ammonites communis, have been figured in Smith's "Strata identified by organized Fossils," collected from the indurated clay nodules of Happisburgh cliff. From clay boulders at Overstrand cliff I have obtained Gryphæa, with unusually thick and gibbous valves. A species of Ostrea or Gryphæa is also found here remarkable for having its valves chalcedonized, and the internal cavity filled with dark-coloured silex. These shells are perfectly white, very thick and tumid, and from four to five inches long. Although I have seen many specimens, all have been considerably rounded by attrition upon the beach, and I do not know of any that have been discovered *in situ* and uninjured.

As these memoranda are the result of a single examination of a portion of our strata which is little known, they will, I trust, be received as such. Subsequent inquiry will doubtless add much to the geologic information here collected, and will probably occasion another communication, in some future Number of the Philosophical Magazine, from

Norwich, Aug. 14, 1822. RICHARD TAYLOR.

XXIII. *Notices respecting New Books.*

PART I. of The Philosophical Transactions of the Royal Society of London, for 1822, has just appeared: the following are its contents:

I. The Bakerian Lecture. An Account of Experiments to determine the Amount of the Dip of the Magnetic Needle in London, in August 1821; with Remarks on the Instruments which are usually employed in such Determinations. By Captain Edward Sabine of the Royal Regiment of Artillery, F.R.S.—II. Some Positions respecting the Influence of the Voltaic Battery in obviating the Effects of the Division of the Eighth Pair of Nerves. Drawn up by A. P. Wilson Philip, M.D. F.R.S. Edin. Communicated by B. C. Brodie, Esq. F.R.S.—III. On some alvine Concretions found in the Colon of a young Man in Lancashire, after Death. By J. G. Children,

dren, Esq. F.R.S. &c. &c. Communicated by the Society for promoting Animal Chemistry.—IV. On the concentric Adjustment of a triple Object-glass. By William Hyde Wollaston, M.D. V.P.R.S.—V. On a new Species of Rhinoceros found in the interior of Africa, the Skull of which bears a close Resemblance to that found in a Fossil State in Siberia and other Countries. By Sir Everard Home, Bart. V.P.R.S.—VI. Extract of a Letter from Captain Basil Hall, R.N. F.R.S. to William Hyde Wollaston, M.D. V.P.R.S. containing observations of a Comet seen at Valparaiso.—VII. Elements of Captain Hall's Comet. By J. Brinkley, D.D. F.R.S. and M.R.I.A. and Andrew's Professor of Astronomy in the University of Dublin. In a Letter addressed to W. H. Wollaston, M.D. V.P.R.S.—VIII. On the electrical Phænomena exhibited in vacuo. By Sir Humphry Davy, Bart. P.R.S. —IX. Croonian Lecture. On the anatomical Structure of the Eye; illustrated by microscopical Drawings executed by F. Bauer, Esq. By Sir Everard Home, Bart. V.P.R.S.— X. A Letter from John Pond, Esq. Astronomer Royal, to Sir Humphry Davy, Bart. President of the Royal Society, relative to a Derangement in the Mural Circle at the Royal Observatory.—XI. On the finite Extent of the Atmosphere. By William Hyde Wollaston, M.D. V.P.R.S.—XII. On the Expansion in a Series of the Attraction of a Spheroid. By James Ivory, M.A. F.R.S.—XIII. On the late extraordinary Depression of the Barometer. By Luke Howard, Esq. F.R.S. —XIV. On the anomalous magnetic Action of hot Iron between the white and blood-red Heat. By Peter Barlow, Esq. of the Royal Military Academy. Communicated by Major Thomas Colby, of the Royal Engineers, F.R.S.—XV. Observations for ascertaining the Length of the Pendulum at Madras in the East Indies, Latitude 13° 4′ 9″,1 N. with the Conclusions drawn from the same. By John Goldingham, Esq. F.R.S.—XVI. Account of an Assemblage of Fossil Teeth and Bones of Elephant, Rhinoceros, Hippopotamus, Bear, Tiger, and Hyæna, and Sixteen other Animals; discovered in a Cave at Kirkdale, Yorkshire, in the Year 1821: with a comparative View of five similar Caverns in various Parts of England, and others on the Continent. By the Rev. Wm. Buckland, F.R.S. F.L.S. Vice-President of the Geological Society of London, and Professor of Mineralogy and Geology in the University of Oxford, &c. &c. &c.—XVII. Communication of a curious Appearance lately observed upon the Moon. By the Rev. Fearon Fallows. In a Letter addressed to John Barrow, Esq. F.R.S. —XVIII. On the Difference in the Appearance of the Teeth and the Shape of the Skull in different Species of Seals. By Sir Everard Home, Bart. V.P.R.S.

A practical Essay on the Strength of Cast-Iron, intended for the Assistance of Engineers, Iron Masters, Architects, Millwrights, Founders, Smiths, and others engaged in the Construction of Machines, Buildings, &c. containing practical Rules, Tables, and Examples; also an Account of some new Experiments, with an extensive Table of the Properties of Materials. Illustrated by four Engravings. By Thomas Tredgold, Civil Engineer; Member of the Institution of Civil Engineers; Author of Elementary Principles of Carpentry; the Article JOINERY in the Supplement to the *Encyclopædia Britannica*, &c. 8vo. pp. 192. 12*s*.

A work of the nature which Mr. Tredgold has now produced has long been a desideratum with English engineers; and it is but justice to say that the author has well executed the task he had imposed on himself.

The work is divided into seven sections:

The first section consists of introductory remarks on the use and qualities of cast-iron, and of cautions to be observed in employing it. This section is followed by two extensive tables, which will often save the practical man a considerable share of trouble in calculation.

The second section explains the arrangement and use of the tables which precede it.

It is a common and a well understood fact, that an uniform beam is not equally strained in every part, and therefore may be reduced in size, so as to lessen both the strain and the expense of material.

The third section points out the value of cast-iron in this particular, and the forms to be adopted for different cases.

The fourth section contains a popular explanation of the strongest forms for the sections of beams; the construction of open beams; and the best form for shafts. A due consideration of these two sections will enable the young mechanic to guard against some common errors in attempting to apply these things to practice.

The fifth section is wholly devoted to experiments; it will be found to contain, in addition to the author's own experiments, almost all of the experiments on cast-iron that have been described by preceding writers. Those he has tried for the purpose of establishing rules, to apply in practice, have been made with a different view of the subject from that entertained by preceding experimentalists; one better adapted for practical application, one which shows that, within the proper limits, our theory of the strength of materials is to be depended upon; but that beyond these limits materials should never be strained in constructions of any kind whatever. Nevertheless it would be extremely desirable that some accurate experiments on the extension of bodies should be made, when the strain exceeds the elastic force; as by that means something important regarding the ductility of matter might be discovered; and perhaps they might throw some light on the nature and arrangement of the ultimate particles of bodies.

In the sixth section he has shown how to obtain some of the most useful practical rules from the first principles that are furnished by experience. He has conducted the investigation of these rules in a manner somewhat different from other writers, and has avoided the use of fluxions. Several new cases are investigated, and some addition is made to the theory of resistance; the reader will find examples of this in treating of the strength of beams, art. 77 to 85; the deflexion of beams, art. 90 to 93; the strain upon beams, art. 96 to 104; the resistance to torsion, art. 222 to 227; and the resistance of columns, art. 230 to 246.

In the seventh section he has considered the resistance of beams to impulsive force. In this section will be found many important rules, with examples of their application to the moving parts of engines, bridges, &c.; wherein the advantage gained by employing beams of the figures of equal resistance is shown.

The seventh section is followed by an extensive Table of the Properties of Materials, and other Data, often used in Calculations, arranged alphabetically. By means of this table the various rules for the strength of cast-iron, contained in the work, may be applied to several other kinds of materials.

A note, which the author has added at the end of the table, on the chemical action of some bodies on cast-iron, will be read with interest by those who employ cast-iron where it is exposed to the action of sea-water.

Each plate is accompanied by a page of descriptive letterpress opposite to it, with references to the articles which the figures are intended to explain.

And, in general, it will be found that the examples are selected with a view to explain the practical application of the rules; and to make the reader aware of the limits and precautions to be attended to. In fact, the want of such information has often brought theory into discredit with some men, whereas the fault ought to have fallen on the person that misapplied it.

In a note, the author acknowledges himself greatly indebted to Dr. T. Young for showing the necessity of attending to the strain which produces permanent alteration on the materials employed.—*Nat. Phil.* vol. i. p. 141.

In another note the author, speaking of his reasons for avoiding the use of fluxions, expresses himself thus:

"I have rejected fluxions in consequence of the very obscure manner in which its principles have been explained by the writers I have consulted on the subject. I cannot reconcile the idea of one of the terms of a proportion vanishing for the purpose of obtaining a correct result; it is not, it cannot be good reasoning; though, from other principles, I am aware that the conclusions obtained are correct. If the doctrine of fluxions be freed from the obscure terms, limiting ratios, evanescent increments, and decrements, &c. it is, in reality, not very difficult. If you represent the increase of a variable quantity by a progression (as is done in art. 249. sect. vii.) the first term of that progression is the same thing as what is called a fluxion; and the sum of the progression is the same as a fluent. A fluxion is, therefore, the first increase of an increasing variable quantity; and the last decrease of a decreasing one, and the expansion of a variable quantity into a progression, is the best and most clear comment that can be added to the lemmas of Sir Isaac Newton."

We cannot too strongly recommend this work to the attention of architects and professional engineers. There is not a useless word in it from beginning to end.

An Introduction to the Study of Fossil Organic Remains, especially of those found in the British Strata: intended to aid the Student in his Inquiries respecting the Nature of Fossils, and their Connexion with the Formation of the Earth. With (10) *illustrative Plates.* By James Parkinson, Fellow of the Royal College of Surgeons, Member of the Geological Society of London, the Wernerian Society of Edinburgh, and of the Cæsarian Society of Moscow. 352 pages, 8vo.

The author of this useful little volume, well known by his previous labours on the interesting branch of science embraced in his title-page, in his preface dedicates the present pages "to the service of those admirers of fossils who have not yet entered into a strict examination of the distinctive characters of these interesting substances."

In this slight but comprehensive sketch the author points out, with more precision than he takes credit for, the difference of forms and structure in the numerous organized beings with which the earth was peopled before the creation of man; marking the several circumstances in which they agreed with, or differed from, the inhabitants of the present world; and points out, from the strata in which they exist, the order in which they, probably, were formed; avoiding however—indeed his limits obliged him—a full statement of those minute distinctions which are the objects of research of the more advanced inquirer; but enough is stated to enable the student to detect the more decided and more important characters of these substances, and to place them under their appropriate genera; so that we hesitate not to state that the work will be found a useful Vade Mecum for the intelligent traveller who may not yet have attempted these inquiries.

The Naturalist's Guide for collecting and preserving all Subjects of Natural History and Botany, intended for the use of Students and Travellers, by W. Swainson, F.R.S. and L.S. This is a very well executed and useful work, as might be expected from the well known experience and zeal of the author.

The Exotic Flora: containing Figures and Descriptions of new, rare, or little-known Exotic Plants; by W. J. Hooker, LL.D. &c. Part I. Royal 8vo.

An Epitome of Chemistry, wherein the Principles of that Science are illustrated in 1000 Experiments; by the Rev. J. Topham, M.A. 12mo. 3*s.* 6*d.*

A New and Classical Arrangement of the Bivalve Shells of the British Islands; by W. Turton, M.D. 4to. With Twenty Plates, drawn and coloured from original specimens in the author's cabinet. 4*l.*

A View of the Present State of the Scilly Islands: exhibiting their vast Importance to Great Britain, and the Improvements of which they are susceptible; by the Rev. George Woodley. 8vo. With a Chart. 12*s.*

Hortus Anglicus; or, the Modern English Garden; arranged according to the System of Linnæus; with Remarks on the Properties of the more valuable Species. 2 Vols. 12mo. 16*s.*

The Study of Medicine, comprising its Physiology, Pathology, and Practice; by John Mason Good, M.D. 4 vols. 8vo.

A System of Anatomy for the Use of Students of Medicine; by Caspar Wistar, M.D. 2 vols. 8vo. 30*s.*

Zoological Researches in the Island of Java, &c. With Figures of Native Quadrupeds and Birds; by Thomas Horsfield, M.D. No. IV. 4to. 21*s.*

Preparing for Publication.

A Translation of Legendre's Elements of Geometry is in the press, and will be published in a few weeks. It will be edited by Dr. Brewster, under the sanction of M. Le Chevalier Legendre, who has communicated several important additions. The diagrams are engraven on wood, so as to accompany the propositions—

propositions—a great superiority over the original work, where they are given in copper-plates at the end of the book.

Mr. Hogg has in the press, a new Edition, with considerable improvements, of his "Concise and Practical Treatise on the Growth and Culture of the Carnation, Pink, Auricula, Polyanthus, Ranunculus, Tulip, and other Flowers."

Sylva Britannica; or Portraits of Forest Trees in different parts of the Kingdom, remarkable for their size, beauty, or antiquity, to be drawn and etched by J. G. Strutt, will speedily be published.

ANALYSIS OF PERIODICAL WORKS ON ZOOLOGY AND BOTANY.

☞ In our future Numbers we hope to devote a larger portion of our attention to ZOOLOGY and BOTANY than we have hitherto done, and give our readers an abstract of the contents of such periodical works as relate to these two branches of science. In the first, there are very few; but in the second, the press is more prolific; and from both we shall subjoin to our report (for which we are indebted to able correspondents) the characters of such new genera as have been proposed, and such new species as are for the first time made known.

Swainson's Zoological Illustrations. No. 23.

This work continues to support its high reputation, both on account of its copious supply of new and interesting subjects, and of the able manner in which they are illustrated by the pen and pencil of the author.—To the ornithologist, this is a very rich number, as it contains four plates of birds, two of which are new, and the others of species but little understood.

Trochilus ensipennis. T. aureo-viridis; mento juguloque cæruleo-violaceis; rectricibus paribus; alis falcatis remigum primorum scapis dilatato-compressis.—HABITAT Amer. Merid.

Platyrhynchus cancromus; the female bird, a very curious species, thus characterized: P. suprà olivaceo-fuscus, infrà pallidè fulvus; jugulo albo; genis pennisque spuriis nigris; strigâ ante et pone oculum, maculâque auriculari albentibus. Hab. in Brasilia. The other birds are: *Ramphastos dicolorus* and *Muscipeta barbata*. Mr. S. appears to have paid much attention to the generic characters of *Platyrhynchus* and *Muscipeta* Cuv. (*Muscicapa* Linn.), and from his observations, it appears these generic groups can only be separated by characters in some degree artificial.

The only conchological subject, is the following new and very beautiful cone. *Conus pulchellus*: testâ aurantiacâ, fasciis albis interruptis ornatâ; spiræ subdepressæ, anfractibus su-

suturam

turam juxta simpliciter sulcatis; suturâ alveatâ; base granona. From Amboyna.

Sowerby's Genera of Shells. No. 7.

No letter-press accompanies this number; the plates of which refer to the genera *Chama. Isocardia* Lam. (the type of which is the Linnæan *Chama Cor.*) *Iridina* Lam., *Solemya* Lam., *Limnea* and *Physa.*

From the two latter plates it appears Mr. G. B. Sowerby proposes to unite the genera *Limnea* and *Physa.* This however will not be consistent with the structure of their respective animals, which are widely different; and we would rather see a greater adherence to the original definitions of Cuvier and Lamarck in an elementary work of this nature.

Mineral Conchology by Mr. J. Sowerby. No. 63.

We are glad to see this interesting and valuable work continued with so much spirit. In this number Mr. Sowerby apparently very accurately figures three species of the modern genus *Cancellaria,* and gives their English (why not Latin?) specific distinctions, and a generic character. This latter appears to us not so concise or clear as might be wished; and we would rather have seen a transcript of Cuvier or Lamarck's definition, particularly as the genus was instituted by the latter. The species consist of *C. quadrata, evulsa,* (*Buccinum evulsum* of Brandon,) and *læviuscula.* All from Barton and Hordwell cliffs in Hampshire.

Pl. 362. *Corbula nitida* and *cuspida,* from the Isle of White, and *C. complanata,* from the crag at Roydon. Three species apparently undescribed; likewise *Mya gregaria,* an unequivalve shell, which, if ascertained to be fluviatile, must, as Mr. S. observes, be removed from this genus.

Pl. 363. *Mya arenaria,* apparently the same as the recent species.

Pl. 364. *Ostrea carinata,* with six well drawn and interesting figures of this curious fossil shell.

The Monthly Botanical Works are as follows:

The Botanical Register. This has the very great advantage of being enriched by the contributions of our illustrious botanist Robert Brown, Esq., and possesses a decided excellence in the ample dissections given of the generic characters, and the copious descriptions of the species. The 90th number contains Pl. 641. *Caladium odorum,* a genus separated from *Arum* by Mr. Brown. Pl. 642. *Brachysema undulatum.* The second species of this genus, yet known, recently brought from New South Wales, characterized as follows: B. foliis ellipticis undulatis mucronatis, vexillo oblongo, cordato, suprà convoluto, obtusato. Pl. 643. *Melca sempervirens*, Wild. Pl. 644. *Melastoma heteromalla*, a new species of this extensive group figured on a double plate. Sp. Ch. M. foliis cordato-ovalibus integerrimis petiolatis subtus flocculoso-lanatis; petalis obcordatis, antheris basi arcuatis. *Don. MSS.* Pl. 645. *Polygala latifolia*, another new and very elegant species from the Cape. *Pol.* (div. Cristatæ) fruticosa, ramis pubescentibus, foliis decussatis subcoriaceis glauciusculis nervosis rhombeo-ovatis, oblatis, suprà nudis, infrà villosiusculis, racemis umbellatis, antheris barbâ longâ rarâ divaricatâ ad basin. Pl. 646. *Marica iridifolia*, a small but elegant species. We wish to see the beautiful natural order of *Ensatæ* more fully illustrated in this work, since it is well known that the gentleman by whom it is at present conducted, has paid long and very particular attention to these plants. Pl. 646. *Stenochilus maculatus*; this is an elegant and very singular plant, recently brought from New South Wales, distinguished thus: *Sten.* caule ramisque sericeis erectis, foliis spathulato-v. ligulato-lanceolatis, flore plurimum brevioribus, pedunculis flexuoso-declinatis; staminibus paulò exsertis.

Curtis's Botanical Magazine. No. 427. Price 3s. 6d.

P. 2338. Hybrid productions are more interesting to the florist than to the botanist; we should therefore wish to see them excluded from this, as well as the last-mentioned work. It must be, however, confessed, that the one on this plate (formed of *Crinum capense et erubescens*) is a very fine plant. At Pl. 2337, we have the same species of *Melastoma* as appears in this month's Register; a coincidence not to the advantage of either of the works, or to the public. If the dissections had been added, we should prefer the present figure, as being more like the plant. Following this is *Hibbertia dentata*, Brown and Decand. In this article Dr. Sims pays a most flattering, but truly merited, compliment to the eminent talents of Mr. Brown.

Burchellia bubalina (*capensis* of Brown, in Bot. Register, Pl. 466.), a genus named after that enterprising traveller and eminent botanist Mr. Burchell. We do not agree with Dr. S. on the impropriety of changing *specific* names when a plant is, by common consent, removed to another genus, provided the change will be a decided improvement. Pl. 2348. The white variety of *Fumaria cava*; and the next (a double plate) contains an elegant figure of *Polerium caudatum* of Willd. with an interesting outline of the whole plant.

Geraniaceæ, or the Natural Order of Geraniums, by R. Sweet, F.L.S.

This work belongs more to the florist than the botanist; for out of the four plates in this number, only one represents a real species, the *Pelargonium Cotyledonis* of Willd.

This plant is formed into a new genus by the name of *Isopetalum Cotyledonis*, of which the following are the characters:

GEN. CH. Cal. 1-sepalus, 5-partitus, lacinia suprema desinente in foveolum nectariferum. Pet. 5 æqualia rugosa. Stam. tubo brevissimo: 5—6 fertilia patentia apice incurva; sterilia inæqualia subulata incurva.——SPEC. CH. I. pedunculis proliferis: umbellis compositis, foliis cordatis peltatis rugosis pubescentibus subtus tomentosis, caule crasso carnoso.

In a work like the present, intended more for amateurs, it would have been much better had the Latin characters been rendered into English; and those given to the mule plants left out altogether, as being equally useless both to the botanist and to the cultivator.

A Monograph of the British Grasses, by G. Graves, F.L.S. No. 1.

This is a very useful and a very cheap publication, got up with great neatness, and the plates on much better paper than the last three works. Indeed, while on this subject, we would strongly recommend the publishers, both of the Register and Magazine, to pay a few shillings more *per ream* for their plate paper; for the beauty of the figures is greatly lessened by the letter-press being seen through the thin paper they now use for the plates. Of this work, the first number contains twelve plates, at the price of 4*s.* 6*d.* plain, or 6*s.* coloured. The descriptions are intelligible to every one, and the work altogether promises to become a particularly useful one, as being within the reach of the farming community.

Loddiges' Botanical Cabinet. No. 64. Price 2s. 6d.

It is a great pity this neat and moderate priced little work does not contain either botanical characters or synonyms; these additions would render it as valuable to the botanist, as it now is to the cultivator: From this cause we are unable to ascertain those which are already described. The designs and the engraving of the plates are very neat and accurate, but we think the portions of each which are coloured, should be done with more care. We should then strongly recommend this work to young persons fond of flower drawing, as offering them a pleasing and an improving occupation in finishing and colouring the plates, according to the portion which is done in each.

Flora Londinensis.—We are happy to learn that arrangement ments are made for the continuance and regular appearance of this national work on the Indigenous Botany of this Kingdom; and from the eminent and well-known talents of the Editor, Dr. Hooker, Regius Professor of Botany at Glasgow, the public may rest with confidence on the intrinsic merit it will possess.—The last number of the New Series contains the following: *Osmunda regalis—Myosotis alpestris—Aristolochia Clematitis—Cochlearia officinalis—Melampyrum sylvaticum—Cheiranthus Cheiri.*

Dr. Hooker's *Exotic Flora* has not come to our hands, but will be noticed in our next.

We regret that a suspension has occurred in the publication of Mr. Lindley's *Collectanea Botanica.* We hope this has only been occasioned by temporary circumstances. It is a work highly interesting to the scientific botanist, and reflects both credit and honour on the author; the plates contain the most ample dissections of the essential parts of each flower; the descriptions are copious, and interspersed with many interesting observations. The intention of the work is to include such only of our botanical novelties as have not already appeared in the publications of the day.

XXIV. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES IN PARIS.

M. Geoffroy St. Hilaire stated that an animal from Senegal, called *Grépart* by Buffon, and *Felis jubata* by Linnæus, had arrived live at the *Jardin des Plantes.*

M. du Petit Thouars read a memoir entitled "New Observations on the removal of a complete circle of bark."

M. Arago announced that the earthquake which took place in the western part of France was also felt in Paris. The register of magnetic observations at the observatory, under the 31st of May 1822, at a quarter past eight o'clock in the morning, states as follows: "The needle oscillates rapidly, and like a pendulum, from east to west: consequently, the reason of this motion is independent of magnetism."

M. Cuvier read a memoir on a new genus of fossil animals raised from the coal-mines of Cadibona, near Savona, which he calls *Antracotherima.*

M. Duméril reported on M. Sigalas's memoir; according to which, the mesenteric veins are endowed with the absorbent faculty for certain substances other than chyle, as M. Magendie had before stated. The memoir has been approved.

M. Am-

M. Ampère read some new Calculations on the mutual action of two Voltaic conductors.

M. Brongniart made a Report on M. Prevost's Memoir, in which he establishes the number, the characters, and the order of superposition of the different *depôts* which have followed one another between the primordial and recent formations (*terrains*) in the environs of Paris, in a great part of Europe, perhaps even in the whole world. The memoir will be printed in the *Recueil des Savans Etrangers.*

M. Geoffroy read some observations, by which he endeavours to prove that the *Monotreme** are oviparous, and that they ought to form a 5th class in the circle of vertebrated animals.

LINNEAN SOCIETY OF PARIS.

June 13.—M. Thollard, of Tarbes, informed the Society, that he had observed a ray of the sun after a shower of rain of a certain duration, falling upon an ear of rye, was sufficient to cause the membrane of the anther inclosing the small vessel containing the pollen, to burst like a pod. This phenomenon may render our information concerning the smut complete. Experiments on this subject should be encouraged.

M. Persoon gave an account of the work of M. Thore de Dax, on the esculent and the poisonous Fungi of the department of Landes. He enlarged on the merits of the work, noticed particularly some species which appear to be quite new, and mentioned the treatment which the skilful physician employs to prevent their poisonous effects.

July 4.—M. Vallot, of Dijon, presented a paper on the *Sphæria repens*, and is of opinion that the sinuous lines observable on the leaves of the rose-bush are not owing to the presence of a parasitic plant, but rather to the ravages of the larva of a species of moth called by him *Phalæna tinea rosella.*

Two memoirs by M. Borghers were read: the first, "An analysis of the means to be employed in making artificial and permanent Meadows in those districts where that important mode of Agriculture is unknown." The other, "Observations on Grafting."

M. Thiebaut de Berneaud read his "Inquiries concerning the plants known to the ancients by the name of *Cytisus.* He therein combats the opinion which would recognise in it the tree medick, the *Medicago arborea*; and proves, by a comparison of the words used by old authors with the properties of an Alpine shrub to be met with every where, that the true *Cytisus*, cried up by Greek and Roman agriculturists, is our common laburnum, *Cytisus Laburnum.*

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* An order including *Ornithorhynchus* and *Echidna.*

XXV. *Intelligence and Miscellaneous Articles.*

ON THE ABERRATIONS OF COMPOUND LENSES AND OBJECT-GLASSES.

A very valuable paper on this subject, by J. F. W. Herschel, Esq. F.R.S. &c. which was read before the Royal [illegible]ciety in March 1821, appeared in Part II. of the Philosophical Transactions for last year, the important object of which is to render the abstruse researches of celebrated geometers who have occupied themselves with the theory of the refracting telescope, practically available for the construction of good instruments: and to present, "under a general and uniform analysis, the whole theory of the aberrations of spherical surfaces, by furnishing practical results of easy computation to the artist, disentangled from all algebraical complexity, and applicable, by interpolations of the simplest possible kind, to all the ordinary varieties of the materials on which he has to work."

The length of the paper, and the pressure of more recent matter, will not permit us now to insert it. It may, however, be useful to present our readers with the practical result in the words of Mr. Herschel himself.

"We may announce it as a practical theorem, which in all probability will be found sufficiently exact for use, that a double object-glass will be free from aberration, provided the radius of the exterior surface of the crown lens be 6·720, and of the flint 14·20, the focal length of the combination being 10·000, and the radii of the interior surfaces being computed from these data, by the formulæ given in all elementary works on optics, so as to make the focal lengths of the two glasses in the direct ratio of their dispersive powers." We are happy to learn that the intelligent author, whose words we have quoted, is about to extend his inquiries on this subject.

VOYAGE OF SURVEY AND DISCOVERY.

Towards the close of last year an expedition was fitted out from Deptford, consisting of the Leven and Baraceuta, from which accounts have been lately received, announcing that on the 28th of May they were about to proceed on the further objects of their voyage. The Persian Gulf and the Red Sea were to be particularly explored and surveyed. At Rio Janeiro a small vessel was purchased to add to the squadron. The whole is under the command of Capt. W. F. W. Owen, a gentleman every way qualified for such a voyage, assisted by proper officers, with a number of young gentlemen destined for future objects of the same nature. No necessary expense was spared in fitting out this expedition with all the requisite instruments of modern science. They were principally furnished by Mr. Thomas Jones, mathematical instrument-maker to the Board of Ordnance; and these, as we learn by a letter which we have seen from one of the officers, have been found very accurate and exceedingly useful.

NORTH POLE EXPEDITION.

No accounts have recently been received from either the naval or overland expedition towards the polar regions. We are happy however to say, that no fears are entertained respecting the result of either. Before next Christmas we expect to be able to lay before our readers favourable accounts from both of them.

FRENCH VOYAGE OF DISCOVERY.

The Coquille corvette, commanded by M. Duperrey, lieutenant-de-vaisseau, the fitting out of which has occupied some months at Toulon, sailed from that port on the 11th of the present month (August). She is about to undertake a voyage from which results interesting to the progress of geography and physical science may be expected.

The Coquille will first sail for the Cape of Good Hope. She will afterwards proceed to the Great Archipelago of Asia, several parts of which she will explore. She will also visit the points of the western coast of New Holland, which were observed towards the end of the last century and the commencement of the present, by Rear-admiral Entrecasteaux, and Captain Baudin; and after putting into some of the islands of the Pacific Ocean discovered by Cook and Bougainville, she will return to France by doubling Cape Horn.

M. Duperrey is to avail himself of all the favourable circumstances which this long voyage may present, to make different observations relative to the configuration of the globe, the inclination of the needle, &c.

Several members of the Academy of Sciences and the Bureau of Longitude have manifested their zeal in communicating to him instructions for that purpose.

No means which could ensure the success of this expedition have been neglected. The corvette has been fitted out with particular care. The crew consists of picked seamen. Letters of recommendation are furnished to the commanders of such foreign establishments as the Coquille may visit. Finally, the zeal of all the superior officers affords reason to hope that the mission intrusted to them will be executed in the most satisfactory manner.

AMERICAN

AMERICAN EXPEDITION TO THE SOUTH.

The expedition under General William Walker and Colonel Joshua Child has terminated happily as regards the safety of the company, having landed on the rived Brassos without the loss of a man.—On the route, considerable time was spent in exploring the coast from the Bay of Atchafalia to the mouth of the Brassos. They passed through Plaquemine into Berwick's and Atchafalia Bays, through Vermilion Bay, touching at the Quelquechu and Sabine, and entered the Bay of St. Bernard, near the east end of Culebra (or Snake Island), which lies in front of, and covers the bay. They then proceeded westward by the mouth of the Trinity, Santa Jacintha (or St. Hyacinth), Cedar, Chocolate, and several other rivers, all of which will afford considerable settlements. After ascertaining that the Brassos did not empty into the Bay of Saint Bernard, as is represented by most maps, they passed into the Gulf through the south-west pass, at the south-west end of the aforementioned island, and found the Brassos made into the Gulf at right angles, without any bay of consequence. The water on the bar was found at very low tide to considerably exceed seven feet, and may be calculated generally from nine to twelve. On the outside of the bar the depth of water is very considerable, sufficient to float vessels of any size. The tide continues for forty miles up, fifty or more feet above low water. The company ascended the river 170 miles, 100 of which they were accompanied by a sloop containing families; which families still continue with the company, all of whom have located themselves on the river banks, and are severally engaged in cultivating gardens, raising corn, &c.

One hundred miles up the river there is a settlement of 30 or 40 persons, most of whom were intended as Mr. Austin's settlers. Sixty miles by water, and thirty by land, above the settlement formed by Messrs. W. and C. there is a considerable number of families, many of whom are wealthy and respectable gentlemen from Louisiana, and other states, who have quitted some of the best planting establishments, and have taken upon themselves to encounter the difficulties and privations incident to settling new and frontier countries. The valley of the Brassos, or rather the timber land near the coast, (for but little difference exists as to the height of the country on the river, and that which is in the rear, and constitutes the *prairie*,) is in general from three to twelve or more miles in width, and covered with that description of timber common on rich lands: viz. ash, walnut, cherry, hickory, and lynn, &c. ree from inundation, though from the several strata it is certainly alluvial. This river can be navigated by keels and barges without any improvement in all seasons; and it is confidently believed by persons best acquainted with it, that steam-boats of moderate, perhaps any size, may pass with convenience for half the year for many hundred miles up. From the central position of this river, the extent, and excellence of its soil,[*] the variety of climate, the facilities with which articles for new settlements can be imported, and of exporting whatever may be offered for exportation by the country, it will warrant the opinion that it is destined to be the most important part of that delightful and interesting province; and but little doubt exists, that as soon as there are settlers sufficient for their own protection and for subsisting the public functionaries, the government will be administered at some point on that river. There were at the time Gen. W. left the Brassos, a number of families and other persons at the mouth of the river, on their way to join the settlers, &c. Colonel Child is the person sent to the government, as agent for the company, &c. The Spaniards manifest the most sincere friendship for the Americans, and wish them not only to occupy the unsettled country, but even to live immediately among them. The Indians also show every mark of friendship.

NATURAL HISTORY.

The Prussian naturalists Dr. Ehrenberg and Dr. Hemprich, on their travels in the North of Africa, arrived on the 15th of February at the celebrated city of Dongola, the capital of Nubia. Previously, in the years 1820 and 1821, they had sent ten chests and four casks, with subjects of natural history, to the Royal Museum at Berlin.

RESEARCHES RELATIVE TO INCUBATION, BY M. GEOFFROY ST. HILAIRE.

Some very interesting researches have. we learn, lately occupied the attention of M. Geoffroy St. Hilaire, relative to all the facts of incubation. They have been carried on by disturbing hens and their affinities in the operations of *laying* and of *hatching*. Persuaded that it is one of the most efficacious means of drawing from Nature some of her secrets in the formation of organized beings, he has contrived to employ her powers so as to make her produce irregularly: counteracting the regular operation of the *nisus formativus*, and requiring Nature to create under circumstances so modified, that her powers are employed for results negative, indeed, as regards the production of beings, but instructive as to the anomalous course which she is obliged to take. He has suspended at discretion the usual course of *laying*, causing the retention of an egg for ten or fifteen days beyond the usual time, from which have resulted alterations and pathological effects in the organs. Exercising over the process a complete control, so as to direct it hourly, daily, and weekly, he has graduated the obstacles, and has been able to calculate upon the results obtained, by combining what is owing both to the *nisus formativus* and to the obstacles interposed. The 8th volume of the *Memoires des Professeurs du Museum d'Histoire Naturelle* now in the press is to contain M. Geoffroy's first memoir on his researches of this kind.

USE OF MARE'S MILK IN TÆNIA.

The German physicians have lately remarked beneficial effects from mare's milk in cases of tænia. Dr. Kortum of Stalberg relates the following case in Hufeland's Journal. A lady between thirty and forty years of age had long suffered from tænia, and several attempts to remove it had failed, owing to the patient's great dislike to medicines, which caused every thing of this kind to be instantly rejected by vomiting. Having heard of several individuals that were cured by simply drinking fresh-drawn mare's milk morning and evening, she resolved to give it a trial. Having an opportunity in autumn, she drank two cups in the evening. Soon afterwards violent pains commenced in her bowels, and continued dreadfully severe during almost the whole night. In the morning, however, she took one cup more, after which pains in her bowels followed, but much less severe than before. In a few days a long piece of dead and partly putrid tænia was discharged, and in a short time afterwards another piece with the narrow tapering end of the worm; and with this all the symptoms ceased. This peculiarity of mare's milk is the more remarkable, as that of the cow seems to be agreeable to the worm, and on being drunk merely alleviates the symptoms.—*Philadelphia Journal of Medical and Physical Sciences, No. 6.*

FRENCH INQUIRIES RESPECTING THE EARLY CUTTING OF WHEAT.

The subject of early cutting of wheat has lately been agitated in France: and at the sitting of the Central Society for the Encouragement of Agriculture, Science, &c. of the northern department, on the 26th of June 1822, a Report was read, containing answers to certain questions on the subject. 1st. Is not early-cut wheat more liable to injury from moths, (*la teigne*) weevils, &c. than that cut perfectly ripe? 2d. Will it keep as well in the stack, barn, or granary? 3rd. Is the quality as nutritive and good? 4th. Will it serve as well for seed? 5th. Does not the straw contract a mouldy smell, and thereby spoil it as fodder? 6th. What species of wheat will best bear early cutting? 7th. (which seems but a modification of query 3.) Is it necessary for the grain to be completely ripe, to be the most profitable, as commonly grown?

To these questions answers have been returned by ten correspondents, which, though varying in minor points, are generally in favour of early cutting. One of them (M. Broy) gives the result of three experiments made by him in three different fields, one half of each field having been cut eight days before the other. The balance is greatly on the side of the first cut, as to *produce*. This perhaps was hardly to be expected; though it is well known that in point of *weight* it is almost sure to beat; which may be easily accounted for from the superior thinness and smoothness of the skin;—in fact, the greater proportion of flour to bran: and for this reason the bakers and millers both in France and England invariably prefer it.

The fifth query is not only superfluous, but savours rather of the ridiculous. Is there ever a doubt of hay being the worse for having been cut green? Every practical farmer knows that the greener *straw* is cut, the more it assimilates to *hay*, and consequently the better it is as fodder for cattle. Besides, if the straw was subject to mould, the grain would also be injured, and that would at once settle the question. There is no doubt but early cut straw is better for cattle, for thatching, for manure, for every thing; and this remark particularly applies to wheat at all affected by mildew. By the by, it may to an English farmer appear rather singular that no mention is made of this disease in the French Report; but perhaps it is not so prevalent there as here. If any doubts exist as to the propriety of early cutting wheat in a sound healthy state, there can be none as soon as the least appearance of mildew is visible on the straw. Years ago Mr. Arthur Young laid great stress on this point, and he was right; though he certainly carried his ideas of its advantages to a preposterous length.

To the sixth question, namely, what species of wheat will best bear early cutting?—the answer naturally is,—That species which in general comes quickest to harvest, and is the most liable to shell out: but in this respect there is, in point of fact, very little difference. The French farmers think *white* wheat is perhaps more likely to shell, from the openness of the kosh, than other sorts. On the whole, the general opinion of our foreign neighbours on early cutting agrees with our own, and may be summed up in the concluding words of their Report: "We find considerable advantages in it; we avoid loss by the shedding (or shelling out) of the grain; we obtain a better price for it at market; we prefer it for seed; it gives us a loaf of a finer and better quality; the straw is finer, more nutritive for cattle, tougher for bands, and makes a more durable and rich manure." "Wheat perfectly ripe requires but a slight degree of heat and moisture to make it vegetate" [in the harvest field]; "but, on the contrary, cut early, say twelve days before the usual period, the bands being still green, and the sheaves arranged in the manner we have described," (*i.e.* by hooding or capping) "it is protected from many evils to which it would otherwise be liable."

One observation occurs here, and it is a part of the subject wholly overlooked by our French neighbours. If early cutting has its advantages, it has also its disadvantages. It is well known that wheat dead ripe may be *cut* and *carried*, as they term it in Norfolk; that is (except there be weeds amongst it), it requires not to remain in the field, but may be carted immediately after cutting. On the contrary, early cut wheat *must necessarily* remain from a week to a fortnight before the straw and corn are forward enough to secure it either in the stack or barn. Consequently; the one incurs a risk which the other needs not incur. It is however admitted, that if the method of hooding the sheaves be adopted, that risk, even in bad weather, will be considerably diminished. Some very sensible remarks on this operation occur in the Report; and it would be well if some of our English farmers would adopt the method. We say *some*, because all those in the moister districts *do* adopt it. Those who live in the drier atmospheres of Norfolk, Suffolk, &c. have comparatively so little rain at the period of harvest, that they regard it altogether as a waste of time and labour, and wholly unnecessary: therefore, when a wet harvest does occur, they are utterly unprepared for it, and of course more injury is done to the corn in these districts than (with the same degree of wet) elsewhere. Besides, the benefits of hooding are not confined to a rainy season, they are equally apparent in weather ever so hot and dry. By being alternately exposed to the rays of a scorching sun by day, heavy dews by night, and perhaps occasional showers, the skin of the grain acquires a degree of coarseness and roughness which tend to deprive it of its weight; whereas, if covered by hooding nothing of this kind occurs, and (on the average) a better sample is produced. There is in short nearly as much difference in the sample, between wheat hooded and not hooded, ed, as between wheat cut early and that suffered to stand till it is ripe. It is but fair, however, to observe, in conclusion, that hooded wheat from the sun being excluded ripens slower, and of course requires more time before it is carted than where the sheaves are uncovered.—S. T.

VARIOUS INDICATIONS OF PLANTS WHEN BURNT.

In a treatise on the instinct of plants, or their capability of attracting from the most different mixtures of earth precisely those substances which are proper and necessary to them, Hermbstadt communicates the interesting observation, that the saline contents of a plant may with some experience be guessed at by circumstances attending their burning; viz. in vegetables *burning quietly* their kali is merely combined with vegetable acids, whilst those burning with a hissing noise, like beet roots, leaves of *Anethum graveolens*, *Borrago officinalis*, *Achillea Millefolium*, and the stalks and leaves of *Helianthus annuus*, and *Datura Stramonium*), contain nitre; and those burning with a crackling noise (like *Rumex Acetosa*, *Artemisia Absinthium*, *Lactuca virosa*, and *Leontodon Taraxacum*) contain muriate of potash.

NUMBER OF THE KNOWN SPECIES OF ORGANIZED BEINGS.

From the collections in the Paris Museums, M. Humboldt estimates (*Ann. de Chimie*, xvi.) the known species of plants at 56,000, and those of animals at 51,700, among which 44,000 insects, 4,000 birds, 700 reptiles, and 500 mammalia. In Europe live about 400 species of birds, 80 mammalia, and 30 reptiles, and in the opposite southern zone on the Cape, we find likewise almost five times more birds than mammalia. Towards the equator, the proportion of birds, and particularly of reptiles, increases considerably. However, according to Cuvier's enumeration of fossil animals, it appears that in ancient periods the globe was inhabited much more by mammalia than birds.

FOSSIL REMAINS.

A discovery of fossil remains was recently made at Atwick, near Hornsea (Yorkshire); the portion of a tusk, about thirty-eight inches in length, twenty inches in circumference at the lower end, and weighing four stone two pounds, was dug up. It is of fine ivory, except where slightly decomposed.

ON PYROLIGNEOUS ACID CONTAINING ALCOHOL.

On examining different samples of pyroligneous acid, M. Döbereiner lately found alcohol in two of them obtained from birchwood. Soon after a manufacturer of sugar of lead in Moscow, wrote to inform him, that in rectifying pyroligneous acid he had obtained one-third of alcohol.

CAPACITY FOR CALORIC.

In the Annals of Philosophy, xiii. 463, the discovery of the simple gases having an equal specific caloric (under equal bulks), and of their capacity for caloric being proportional to their capacity for oxygen, is considered as *most important*, and attributed to M. Dulong. This is inaccurate, at least in a considerable degree; for in Meinecke's Explanations of Stochiometry 1817, 8vo. 192 to 201, those laws are precisely established, with several other determinations, and in their connexion with other stochiometric laws. But Messrs. Dulong and Petit have contributed much to the confirmation of those laws by a dissertation read on the 12th of April 1819. As to their importance, further inquiries will decide upon it.

EFFECT OF THE VOLTAIC PILE UPON ALCOHOL.

M. Lüdersdorff, architect at Weissensee, in Prussia, states (in an inquiry communicated by M. Hermbstadt) that he produced an ethereal fluid by the action of the Voltaic pile upon mere diluted alcohol.

The same person has changed a mixture of equal parts of alcohol and solution of ammonia by continued galvanizing into a fluid no longer inflammable, of a wine yellow colour, of a bitterish flavour, and a nauseous smell like cantharides, which having been slowly evaporated, gave a greasy residuum.

SUPPOSED CONDUCTING POWER OF STRAW.

In making experiments in order to ascertain the practicability of straw conductors proposed by Lapostolle, Dr. R. Brandes and Lieut. Holzermann found that a stalk of straw could only discharge a Leyden phial when the straw was moist, and even then only slowly and imperfectly; but by no means when perfectly dry. They therefore conclude that Lapostolle's proposal was to be rejected. This agrees with M. Trommsdorff's very accurate experiments.

OBSERVATORY AT MADRAS.

It is now some years since an observatory was built at Madras; but hitherto, though an astronomer (Mr. Goldingham) every way qualified for the situation has been attached to the establishment, there has been a deficiency of instruments necessary for the making of any observations that could be applied to the purposes of science. This deficiency having been properly represented to the Directors of the Honourable the East India Company, they have, much to their credit, taken the matter into consideration; and it is now expected that the necessary instruments to render the Madras Observatory efficient will soon be supplied, so that we may expect, ere long, to hear that regular observations are taken in that quarter of the British dominions.

CAPT. KATER'S PENDULUM.—FIGURE OF THE EARTH.

Our scientific readers need not be informed how much attention has been, for some time, bestowed upon those branches of science which have for their object the ascertaining the true figure of the earth. But it may not be known to many of them, that much in the furtherance of this object has been made to depend on the results obtained from observations made by means of Capt. Kater's invariable pendulum. To give full efficacy to the knowledge to be obtained by this means, a pendulum made under Capt. Kater's directions was constructed by Mr. Thomas Jones, which has been tried at the different stations of the Trigonometrical Survey by the ingenious inventor himself, and the results obtained thereby have proved exceedingly satisfactory.

In consequence of these results, one of these pendulums was made about two years ago, under the inspection of Capt. Kater, by the same ingenious artist who made the pendulum to which we have alluded, and was sent out to Mr. Goldingham, the astronomer who has the superintendance of the Madras Observatory, who has made many observations therewith, the results of which have agreed with the observations made on the trigonometrical stations to a degree far beyond what could have been expected.

STEAM-VESSELS.

A very interesting experiment has been made of steam-vessels on canals, in the Union Canal at Edinburgh, with a large boat, 28 feet long, constructed with an *internal* movement. The boat had 26 persons on board; and although drawing 15 inches of water, she was propelled by only four men at the rate of between four and five miles an hour, while the agitation of the water was confined entirely to the centre of the canal.

MOTION OF THE ELASTIC FLUIDS.

In the Quarterly Journal 1817, Mr. Faraday has stated that the different gases move through pipes with different velocities under the same degree of pressure; that is, quicker in proportion to their levity. On the contrary, Girard endeavours to prove in the *Annales de Chimie*, xvi. 129. that all elastic fluids under equal circumstances move with equal speed, independent of their specific weight, and that their motion is merely determined by the pressure which acts upon them.

CHRONOMETERS.

It is only within these few years that a due value has been attached to the aids to be obtained from good chronometers in ascertaining the longitude at sea. It is but justice however to say, that since its value was duly appreciated, much attention has been paid to this branch of science, especially by our Board of Admiralty. It is pleasing to be able to state, that they have employed active measures to manifest the value which they attach to this object. To this Board the world is indebted for the eminently valuable labours of Dr. Tiarks, the same gentleman who was employed to settle the boundary line between the United States and the British possessions in North America, who during the intervals of that service was sent with the Owen Glendower frigate to Madeira, with about a dozen of chronometers furnished by different makers of eminence, with an understanding that those chronometers which are found to perform best, will be purchased by the Board of Admiralty at prices proportioned to their various merits. We know no way in which a portion of the public expenditure can be more beneficially applied.

OBITUARY.—Mr. James Dickson.

On the 14th instant died at his house near Croydon, aged 84, Mr. James Dickson, of Covent Garden, Fellow of the Linnæan Society, and Vice-President of the Horticultural Society of London.—We shall be happy to be enabled in a future number to give some account of this eminent and venerable man. In the mean time, the following extract from the Life of his brother-in-law, the celebrated Mungo Park, prefixed to his Journal (Ed. 1815) by the judicious Editor, may be acceptable to our readers. "Mr. Dickson was born of humble parents, and came early in life from Scotland, his native country, to London. For some time he worked as a gardener in the grounds of a considerable nurseryman at Hammersmith, where he was occasionally seen by Sir Joseph Banks, who took notice of him as an intelligent young man. Quitting this situation, he lived for some years as gardener in several considerable families: after which he established himself in London as a seedsman; and has ever since followed that business with unremitting diligence and success. Having an ardent passion for botany, which he had always cultivated according to the best of his means and opportunities, he lost no time in presenting himself to Sir Joseph Banks, who received him with great kindness, encouraged him in his pursuits, and gave him access to his valuable library. He thus obtained the free use of one of the most complete collections of works upon botany and natural history, which has ever yet been formed; and which, through the liberality of its possessor, has contributed in a greater degree to the accommodation of scientific men, and the general advancement of science, than many public establishments. Such leisure hours as Mr. Dickson could command from his business, he devoted to an assiduous attendance in this library, and to the perusal of scientific books obtained from thence. In process of time he acquired great knowledge, and became eminent among the English botanists, and is now known in Europe among the proficients in that science as one of its most successful cultivators, and the author of some distinguished works. At an advanced period of life he is still active in business, and continues to pursue his botanical studies with unabated ardour and assiduity.

"Mr. Dickson is a Fellow of the Linnæan Society, of which he was one of the original founders, and also Fellow and Vice-President of the Horticultural Society. Several communications from him appear in different volumes of the Linnæan Transactions; but he is principally known among botanists by a work entitled "Fasciculi Quatuor Plantarum Cryptogamicarum Britanniæ," Lond. 1785-93; in which he has described upwards of four hundred plants not before noticed. He has the merit of having directed the attention of the botanists of this country to one of the most abstruse and difficult parts of that science, to the advancement of which he has himself very greatly contributed.

"Such an instance of successful industry united with a taste for intellectual pursuits, deserves to be recorded; not only on account of its relation to the subject of this narrative, but because it illustrates in a very striking and pleasing manner the advantages of education in the lower classes of life. The attention of the Scottish farmers and peasantry to the early instruction of their children has been already remarked, and is strongly exemplified in the history of Mr. Park's family. The diffusion of knowledge among the natives of that part of the kingdom, and their general intelligence, must be admitted by every unprejudiced observer; nor is there any country in which the effects of education are so conspicuous in promoting industry and good conduct, and in producing useful and respectable men of the inferior and middle classes, admirably fitted for all the important offices of common life."

LECTURES.

St. George's Medical and Chemical School.—In the first week of October, these Lectures will commence as usual in George-street, Hanover-square, and at the Royal Institution in Albemarle-street, viz.

1. On the Laws of the Animal Œconomy, and the Practice of Physic, with Pathological Demonstrations, by George Pearson, M.D. F.R.S. Senior Physician to St. George's Hospital.

2. On the Science of Chemistry, with the Operations in various Departments, by W. T. Brande, Prof. Chem. Roy. In., Secretary Roy. Soc. &c.

3. On Therapeutics, with Materia Medica, by George Pearson, M.D. F.R.S., &c.

LIST OF PATENTS FOR NEW INVENTIONS.

To Jonas Hobson and John Hobson, of Mythom Bridge, in the parish of Kirkburton, in the county of York, woollen manufacturers and merchants, for a new series of machinery for the better, more effectual and expeditious mode of shearing, cutting and finishing woollen cloths, kerseymeres, and all other descriptions of cloths and piece-goods which require the use of the shears.—Dated the 27th July 1822.—2 months allowed to enrol specifications.

To John Stanley, of Chalton-row within the town of Manchester, Lancashire, smith, for certain machinery calculated for a more efficacious mode of fuelling, or supplying of furnaces in general with fuel, whereby a considerable reduction in the consumption of fuel, the appearance of smoke, and of labour, is effected.—27th July.—6 months.

To John Pearse, of Tavistock, ironmonger, and clock- and watch-maker, for certain improvements in the construction and manufacture of spring jacks, and their connexion with roasting apparatus.—27th July.—6 months.

To Sir Anthony Perrier, of the city of Cork, knt. for certain improvements in the apparatus for distilling, boiling, and concentrating by evaporation, various sorts of liquids and fluids. 27th July.—6 months.

To Robert Roxby, of Arbour-square, Stepney, Middlesex, gent., for certain improvements on or additions to the astronomical instrument known by the name of the quadrant.—31st July.—2 months.

To William Cleland, of the city of Glasgow, in North Britain, gent., for his improved apparatus for the purpose of evaporating liquids.——17th August.—4 months.

METEOROLOGICAL TABLE,

The *London* Observations by Mr. Cary, of the Strand. The *Boston* Observations by Mr. Samuel Veall.

Days of Month. 1822.	Thermometer.				Height of the Barom. Inches.		Weather.	
	London.			Boston.				
	8 o'clock Morning.	Noon.	11 o'clock Night.		Lond.	Boston	London.	Boston.
July 27	58	70	58	71	29·85	29.25	Fair	Fine
28	57	66	56	69	·60	29	Showery	Fine
29	57	70	58	63·5	·57	29·10	Showery	Cloudy
30	56	64	54	56	·64	29·25	Showery	Do. rain
31	55	61	51	60	·75	29·33	Showery	Do. do.
Aug. 1	53	66	50	62·5	·88	29·40	Showery	Fine
2	55	59	50	60	30·01	29·45	Showery	Cloudy
○ 3	54	65	51	64·5	·06	29·61	Fair	Fine
4	55	68	57	60·5	29·87	29·55	Fair	Cloudy
5	55	67	58	64·5	·95	29·50	Fair	Cloudy
6	56	64	62	61·5	30·08	29·65	Cloudy	Cloudy
7	59	69	59	67	·15	29·70	Cloudy	Cloudy
8	55	70	60	72·5	29·95	29·40	Fair	Fine
9	59	68	61	67	·82	29·30	Cloudy	Cloudy
10	60	71	62	65	·86	29·40	Fair	Cloudy
11	61	70	62	73·5	·84	29·40	Cloudy	Cloudy
12	60	70	62	74·5	·90	29·25	Fair	Fine
13	62	72	60	67	·88	29 35	Showery	Fine
14	60	69	60	71·5	·96	29·40	Cloudy	Cloudy
15	59	68	55		·89		Fair	
☾ 16	57	70	67		30·15		Fair	
17	64	76	67		·29		Fair	
18	66	75	61		·24		Fair	
19	61	76	67		·23		Fair	
20	67	74	65		·20		Fair	
21	62	80	69		·05		Fair	
22	60	79	67		29·94		Fair	
23	60	74	60		30·03		Fair	
24	58	66	60		29·89		Cloudy	
25	55	67	55		·79		Showery	
26	55	69			·74		Fair	

The quantity of rain from 31st July amounts to 2·40.

N.B. The Barometer's height in London is taken at one o'clock.

THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL.

30th *SEPTEMBER* 1822.

XXVI. *On the Origin and Discovery of Iron.* By DAVID MUSHET, *Esq.*[*]

TRADITION informs us that the discovery of iron was made in consequence of the accidental burning of a wood in Greece. This is possible, and might be rendered probable, if on examination the remains of iron-mines were now to be found on the sites of some of the ancient forests. But I think the discovery more likely to have been made in the conversion of wood into charcoal, which undoubtedly the Greeks used for culinary if not for chamber purposes.

If the operation of "coaling," as it is called, was in former times at all similar to that now practised, and it could not well be more simple, the discovery was unavoidable, where iron ore was found in quantity, as in this neighbourhood, on the surface of the ground. The wood collier covers his pit or wood heap with a coating of the moistened soil. If this contained fragments of iron ore, many would occasionally be exposed to the contact of heated charcoal, during the combustion necessary to its conversion, and undergo de-oxidation so as to reflect a bright surface, on being struck with a flint or stone. If a mass of ore accidentally dropped into the middle of the burning pile during a period of neglect, or during the existence of a thorough draft, a mixed mass partly earthy and partly metallic would be obtained, possessing ductility, and extension under pressure. But if the conjecture is pushed still further, and we suppose that the ore was not an oxide, but rich in iron magnetic or specular, the result in all probability would be a mass of perfectly malleable iron. I have seen this fact illustrated in the roasting of a species of ironstone, which was united with a considerable quantity of bituminous matter: after a high temperature had been excited in the interior of the pile, plates of malleable iron of a tough and flexible nature were found, and under circumstances where there was no fuel but that furnished by the ore itself. But to return.

Iron being once discovered, many attempts would be made to give to that which was the effect of accident, a more unerring result. These attempts would at first be confined within the precincts of the wood or forest that furnished both the fuel and the ore. Some time would be necessary to determine what particular stone afforded the ponderous result; and when the superior weight of the ore led to a conclusion in its favour, many fruitless attempts would be made to place it under circumstances similar to those in which the discovery was first made. If the soil afforded ores of different colours and qualities, much time would be lost in assigning to each its respective share of merit or demerit, qualities would be imputed, and inferences drawn, that merely resulted from the imperfection of the attempts. If the ores were oxides of iron, and suddenly exposed to a high temperature, the whole would be converted into a heavy black glass, and thrown aside as containing no metal. Those ores, on the contrary, would obtain a decided preference, that contained little earthy matter, and the iron but little in the state of oxide. But it would soon be discovered that these ores, without a violent and continued heat, would come out from the fire but little changed, except upon their surfaces, and incapable of being converted into any form.

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[*] Communicated by the Author.

Ages might pass away, and the fact itself only be known by tradition, when a new accident, in more skilful hands, led to new and more successful attempts. During a period of laborious perseverance, it would be discovered that not only high temperature was necessary, but that the ore thus heated should not be directly exposed to the action of that air which was the source of the temperature: this would lead to the practice of keeping the ore surrounded as much as possible with fuel while the process of its conversion into iron was going forward. The charcoal fire for this purpose would be found inconvenient and expensive: hence would arise the first attempts at a furnace. In the present time the manufacture of iron is not only associated with bellows, but with blowing machines of the most powerful construction; but it is exceedingly probable that bellows existed, and might have been used for the purpose of forging iron, long before they were applied to its manufacture from the ores.

Iron, long after its discovery, was most probably applied solely to agricultural purposes; and it was not until the discovery of a regular method of converting it into steel, or of producing steel from its ores, that it advanced its high claim upon the consideration of mankind; and in our days so powerfully has the justice of this claim been advocated, that the mastery of this metal may be safely acknowledged as a test of the highest advancement of civilization among nations.

The general use of hardened copper, by the ancients, for edge tools, and warlike instruments, does not preclude the supposition that iron was then comparatively plentiful, but confined to the ruder arts of life. The knowledge as well as the mixtures of copper, tin and zinc, seem to have been amongst the first discoveries of the metallurgist. Instruments fabricated from these alloys, recommended by the use of ages, the perfection of the art, the splendour and polish of their surfaces, not easily injured by time or weather, would not soon be laid aside or superseded by the invention of simple iron, inferior in edge and in polish, at all times easily injured by rust, and in the early stages of its manufacture converted with difficulty into forms that required proportion or elegance.

Other reasons may be adduced to show why iron may have existed, and did not supersede the use of copper and brass in the art of war, or for general purposes. Og, king of Bashan, had his bedstead made of iron, which shows that the metal was known at an early period in Palestine; and some of the kings of Canaan had chariots made of iron. Many ages afterwards warlike instruments, requiring edge, continued to be formed from a hardened modification of copper, or tin and copper. The first correct idea we receive from history of the importance of steel in the arts, is from the account of the present made by Porus to Alexander of 40 lbs. of Indian steel; a present which we are bound to consider was the most valuable that Porus could bestow, and the most acceptable that Alexander (at that time overwhelmed with the spoils of the East) could receive. This transaction on the banks of the Hydaspes must have taken place at least 800 years after iron was in use in Palestine, and affords a strong presumption that steel, if not then altogether unknown to the artificers in Alexander's army, was an article exceedingly rare. The same observation will equally apply to India at the same time. Nothing in the estimation of Porus, in his extensive dominions, was more rare or valuable than a gift of steel. If we contrast the native manufacture of steel in India at that time with its present abundance and cost of production, we must infer that the progress of this art has not been stationary in India since the days of Alexander. We learn from Dr. Buchanan's account of iron-making at one particular station in India, that a man's labour for one day is nearly equivalent to the value of two pounds of iron; and in another account, the value of one pound of wook, or Indian steel, is stated to be equal in value to six pounds of iron; so that the labour of a man for three days will at the present time purchase one pound of such steel as Porus deemed his highest gift, and which the conqueror of the world disdained not to accept. In other words, the gift in our times could be purchased by the labour of one man for 240 days, equivalent in our money to 30*l.* or 40*l.*

But to return to the subject of iron,—to trace if possible these probable steps by which it was first placed in the scale of manufactured products. I say probable steps, for I am afraid there does not now exist in this or any other country a clue by which the rise and progress of this art may with certainty be traced, an art which has given man the mastery over all other metals and minerals, and has conferred on him the unrivalled dominion of the universe. Yet whilst the exploits of the conqueror and the intrigues of the demagogue are faithfully preserved through a succession of ages, the persevering and unobtrusive efforts of genius, developing the best blessings of the Deity to man, are for ever consigned to oblivion.

The uncertainty of obtaining iron in the mode by which it was originally discovered, would probably give rise to a specific operation for this purpose. The powerful effects of air in raising the temperature would soon be appreciated, and its current by various contrivances directed upon the burning fuel. The œconomy of the latter, the attenuation and concentration of the heat, would next become objects of consideration; and different rude contrivances with stone and mortar would for a time stimulate the labour and exhaust the patience of the daring metallurgist. From manifold combinations there would at last result a furnace capable of producing uniform results. Greatly delighted must this early worker in iron have been, when he found he could obtain by regular rules of process, that which had only existed by chance. This feeling must have been equally shared with the more early inventors or extractors of other metals, and in many respects enjoyed by them in a superior degree. The production of a crude infusible mass of iron withdrawn from the furnace, burning by its own intense temperature, surrounded by scoria, and compressed to form by repeated blows, would seem a less perfect result than a pure molten stream of brass or copper issuing from the crucible of the founder, and becoming in the mould, by a creation simultaneous with its existence, matter of the most perfect shape and figure.

If, however, we were to appreciate the merits of the respective discoveries by the unwearied and unremitting labour employed in both cases, the palm of excellence would probably be bestowed on the ruder worker in iron. Less ingenuity and contrivance would be apparent; but an uncommon stretch of toil and labour only could have crowned his labours with success.

As soon as certainty of operation was established, other improvements would follow in the art of iron-making. As the properties of the metal were developed, demand would confer an additional value upon the product. The smith, to use a phrase common to this and other countries, would, in proportion as his manufacture increased in value, direct his attention to the "œconomy of the process." In his first assays, both quantity and proportion would most likely be entirely overlooked, and the real quantity of metal in an ore computed only by the crude and imperfect extracts. Habit and observation would in time point out the utility of having a measure of bulk whereby to repeat those proportions correctly, that experience had taught him were productive of the most uniform results. A favourable combination of circumstances in the furnace, might at a fortunate conjuncture enlarge his bloom of iron: this repeated, would lead to a suspicion that more iron might be extracted from the ore, were the manipulation itself more perfect. This would furnish new motives for observation and contrivance. Improvement would follow, and the smith would feel the importance of the result, whether it was equal to one-fourth, one-third or one-half the weight of the ore. It would also be discovered that some ores, however perfectly operated upon, would not yield an equal quantity of iron with others: hence would arise the important distinction of rich and lean ores. This simple fact would give a new impulse to exertion. Rich ores would be sought after with avidity, and paid for in proportion to the quantity of metal obtained from them by the smith. When these ceased to exist in plenty upon the surface of the earth, digging would be resorted to; the art necessary to distinguish between a lean and a rich ore, and the skill necessary to follow them through soil and rock, would become important qualifications. A new class of labourers endowed with superior skill and intelligence would in time be formed, to which has since been given the name of miners. Though we cannot precisely say what was the peculiar form of the iron furnaces or air bloomeries of the Greeks and Romans, yet we may form some idea from what is now practised by other nations in the infancy of the art. Park, I think, in his Travels in the Interior of Africa, has given a drawing of a low conical furnace, which seems an easy and natural structure for a rude age. Small openings at the lower end of the cone to admit the air, and a larger orifice at the top, would, with charcoal, be sufficient to give a considerable degree of heat. The furnace would in the first instance be filled with layers of charcoal and iron ore alternately, and the fire applied to the openings in the lower extremity of the furnace. The measure of heat would easily be regulated by narrowing or enlarging those apertures, and a renewal of the fuel when necessary easily effected by the funnel at the top. This seems the greatest advance in the art which the African has made, and it is highly creditable to the inventive powers of that people, that Africa is ranked in the class of iron-making nations. Mexico and Peru, with all their claims upon civilization, could not at the time of their discovery boast the possession of such an art.

In other countries furnaces of various shapes have been devised; but, however different in structure, they have acknowledged the same practice now followed upon the burning sands of Africa. Oblong furnaces with flues and passages of an enlarged size to conduct and rarify the air, and capable of converting more ore at one time, seem to have been the greatest advancement in the air-bloomery: the practice of which under every contrivance was to stratify the ore with the fuel; and the principle, a gradual de-oxidation of the ore whilst it maintained the combustion. A high temperature united or welded the masses of iron ore together; these were carried to a stone, or in the advancement of the art to an anvil, where, by means of hammers, the unmetallized particles, forming with the earths a vitreous scoria, were forcibly expelled, and form given to the metallic mass. This is now principally confined to some of the Spanish provinces, and to the shores of the Mediterranean, where considerable quantities of ore from the Island of Elba are worked in this manner. The remains of furnaces of this sort have been found in elevated situations in Scotland; and though the form can no longer be traced, yet in the neighbourhood of Dean Forest are the ruins of old furnaces surrounded with scoria, on the high grounds, where the only apparent inducement for their erection could have been a strong current of air.

The practice of these air-bloomeries must have been slow in the extreme. A long and continued cementation of the ore in contact with the fuel, must in the first instance have been necessary to dispose the metallic particles to unite. A too rapid exposure to a high temperature would be apt to unite a considerable portion of oxygen with the ore, which in this way would acquire a considerable degree of fusibility: this would not only diminish the quantity and quality of the iron, but retard the general operation. To render the quality of the iron homogeneous, masses of iron ore would be used as nearly of one size as possible, which would give rise to a rejection, in part, of the small or dust ore generally the richest of the vein. That this practice prevailed to a considerable extent in Gloucestershire, is evident from the large quantities of small mine found from time to time in the old caverns or weeldons of the limestone. These acknowledged evils undoubtedly affected the œconomy and prosperity of the trade, until some more fortunate or more ingenious worker applied the bellows to the art of iron-making, which gave rise to the blast bloomery, and occasioned a great revolution and improvement in the fabrication of that valuable and highly useful metal.

Coleford, Gloucestershire.

[To be continued.]

XXVII. *Suggestions for simplifying Mr.* Ivory's *Solution of the Double Altitude Problem. By Mr.* Edward Riddle, *Royal Naval Asylum, Greenwich.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—In The Philosophical Magazine for August 1821, Mr. Ivory has given a new and direct solution to the well known nautical problem, which requires the latitude to be determined from two altitudes of the sun, and the time elapsed between the observations; and as the rule which he has deduced possesses several advantages over any other that has hitherto been given for the purpose, it may be hoped that teachers of navigation will generally feel it their duty to endeavour as much as possible to introduce it into practice.

I conceive however that, simple and convenient as the rule is, its form yet admits of an advantageous alteration. In the numerical calculation from the formulas which Mr. Ivory has investigated, it is necessary to refer both to a table of *natural sines*, and to one of the *logarithms of numbers*. But in this solution of the problem, the use of both these tables may be very easily dispensed with; as by a simple transformation of two of the expressions the whole of the calculations may be performed with even greater neatness and facility, with the aid of no other table than one of *log. sines*, &c.

In a rule for the guidance of ordinary computers, it is desirable to avoid, when it can conveniently be done, a reference to a variety of tables; as the mere act of turning from one table to another has in itself a tendency to perplex. But by the slight alteration which I have to propose in Mr. Ivory's solution, this objection to it (if an objection it may be called) will be completely obviated; and, besides, the form of calculation admits of an exceedingly convenient arrangement; a circumstance which may recommend the rule to the attention of persons who, otherwise, might not at once be sensible of its value.

In this class of persons I must observe that I feel myself included; for it was not till I had given Mr. Ivory's paper a very attentive consideration, and had been at considerable pains in arranging the calculations of an example, that I fully perceived the great practical importance of this result of his able and ingenious view of the problem.

The expressions to which I have alluded as admitting of a convenient transformation are these; viz.

"$A = \frac{\sin h + \sin h'}{2}$" and "$B = \frac{\sin h - \sin h'}{2}$;" where h and h' are the altitudes, and the *logarithms* of A and B are the quantities required.

Now, $\frac{\sin h + \sin h'}{2} = \sin \frac{h + h'}{2} . \cos \frac{h - h'}{2}$;

and $\frac{\sin h - \sin h'}{2} = \sin \frac{h - h'}{2} . \cos \frac{h + h'}{2}$; expressions from which the logarithms of A and B may be obtained at once.

With these alterations in the forms of the expressions for A and B, Mr. Ivory's formulas for computing the latitude may be arranged as under; t being the half interval of time between the observations, h and h' the altitudes, D the declination, and L the latitude.

1. $\text{Sin } a = \cos D . \sin t.$
2. $\text{Cos } b = \sin D . \text{sect } a.$
3. $\text{Sin } c = \cos \frac{h + h}{2} . \sin \frac{h - h'}{2} . \text{cosect } a.$
4. $\text{Cos } d = \sin \frac{h + h'}{2} . \cos \frac{h - h'}{2} . \text{sect } c . \text{sect } a.$
5. $\text{Sin } L = \cos c . \cos (b \mp d).$

It may be observed on these formulas, that a, sect a, (which is wanted twice,) and cosect a, are obtained at the same opening of the table; and so are cos D and sin D; sin and cos of $\frac{h + h'}{2}$; sin and cos of $\frac{h - h'}{2}$; and c, cos c, and sect c; so that the number of openings of the book of tables is comparatively small.

The demonstrations of the formulas may be seen in Mr. Ivory's paper, which also contains the investigation of a very neat method of estimating the effect of the change of the sun's declination, in the interval between the observations, when it is deemed of any consequence to advert to it. The practical rule from the above formulas may be thus given in words, observing that in every case the tens are to be rejected from the indices of the logarithms.

Practical Rule.

1. Add together the *sine* of half the elapsed time, and the *cosine*

cosine of the declination, and the sum will be the *sine* of *arc first.*

2. Add together the *secant* of arc first, and the *sine* of the declination, and the sum will be the *cosine* of *arc second;* which will be *acute* when the latitude and declination are of the same denomination, but *obtuse* when they are of different ones.

3. Add together the *cosect.* of arc first, the *cosine* of half the sum of the altitudes, and the *sine* of half their difference; and the sum will be the *sine* of *arc third.*

4. Add together the *secant* of arc first, the *sine* of half the sum of the altitudes, the *cosine* of half their difference, and the *secant* of arc third, and the sum will be the *cosine* of *arc fourth.*

5. When the zenith and elevated pole are on the same side of the great circle passing through the places of the sun at the times of observation, the *difference* of arcs second and fourth will be *arc fifth,* otherwise their *sum* will be *arc fifth.*

6. Add together the *cosines* of arcs third and fifth, and their sum will be the *sine* of the latitude.

Note. When the declination and latitude are nearly equal, and of the same name, it may sometimes be doubtful whether the *sum* or *difference* of arcs second and fourth ought to be taken for arc fifth. But the computation is soon made on both suppositions; for *cosine* of arc fifth is the last logarithm which is taken from the tables, and the other parts of the calculation are therefore not affected by the change. One of the results must certainly be the required latitude, and the latitude by account will generally be sufficient to determine which of them ought to be taken. But when the sum of arcs second and fourth is equal to 90°, or greater, it can only be their difference which is arc fifth.

Form of Calculation.

sin H.E.T. ° ′			
sin Dec	cos		
sin Arc I	sect		cosect
Arc II	cos		
		sect Arc I	
		sin ½ sum alts. ° ′	cos
		cos ½ diff. alts.	sin
		sect Arc III ·····	sin
Arc IV	cos	·····	cos
Arc V	··		cos
		Lat.	sin

Example.—Given the altitudes of the sun 19° 41′ and 17° 13′, interval one hour, and the sun's declination 20° S. to find the latitude, it being by account about 50° N.

19° 41′
17 13
Sum 36 54, half sum 18° 27′ half interval 7° 30′
Diff. 2 28, half diff. 1 14

9·115698	sin	7°	30′							
9·972986	cos	20	0	sin	9·534052					
9·088684	sin I	7	3	sect	·003296				cosect	·911030
	II	110	10	cos	9·537348					
		⋮	⋮		·003296					
		⋮	⋮		9·500342	sin	18°	27′	cos	9·977003
		⋮	⋮		9·999899	cos	1	14	sin	8·332924
		⋮	⋮		·006103	sect III	9	35	sin	9·221037
	IV	71	8	cos	9·509640				cos	9·993897
	V	39	2	……	……	……	……	……	cos	9·890298
					Lat.		49	59	sin	9·884195

I have introduced the rule in the above form among the young navigators in this Institution; and if what I have said may be the means of drawing the attention of practical men to the subject, my object in writing these observations will be attained.

XXVIII. *On the Relation of Acids and Alkalis to vegetable Colours, and their Mutations thereby.* By JOHN MURRAY, *F.L.S. M.G.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

July 9, 1822.

GENTLEMEN,—YOU had the goodness to insert in a former Number of the Philosophical Magazine (volume lviii. p. 273) a few remarks of mine on the change of vegetable colours by metallic salts; and it is more than a twelvemonth since I have in my prælections pointed out that the mere change of colour produced by acids and alkalis afforded no certain index of their nature. I showed the action of salts of iron, &c. on syrup of violets, &c. in my experiments at the Surry Institution, both in my public discussions and to several individuals in the laboratory of that Institution.

At page 274 of the Philosophical Magazine, vol. lviii. my language, one would think, is emphatic enough. "It seems evident that we have yet to learn the invariable characteristics of alkalis and acids. We may attempt to cover our ignorance by a free use of the term *anomaly;* but I do hold that in the universe of God there is no such thing as anomaly."

After all this, I do own that Mr. Faraday's paper on the same subject in Mr. Brande's Journal for 1st instant, pp. 315 and 316, surprised me not a little, because I must assume egotism, though it may appear, that the very humble *priority* of requesting the attention of chemists to this important fact is due *to me*, as clearly demonstrated by the paper published in your 58th volume.

Mr. Faraday, however, is not pleased either to advert to that paper or my name as connected with the subject, and his paper concludes in the following language:—"My object has not been to trace these changes *as far as possible*, but merely to show their general appearance, and to guard against any deceptive conclusion with respect to solutions tested by Turmeire, and *to call attention to the distinguishing characters of acids and alkalis*." In the present *very imperfect* state of chemical science we may not be able "to trace these changes *as far as possible;*" and *I* had already *called attention* to the *deceptive conclusions* in question. Mr. Faraday has in a former part of this paper observed, "*I* find on trial, however, so many substances possessing this property," namely, what I had pointed out in the action of subacetate of lead, nitrate and sulphate of copper, &c. "that," continues this ingenious chemist, "it must either be limited more exactly than has yet been done, or else given up as a distinguishing property." How "limited more exactly?" It must be given up entirely.

Mr. Faraday endeavours to *account* for the change, but the success of this attempt does not seem quite so evident; for in one part of his memoir he thinks that it is in consequence of the *protosalts* becoming *persalts*, though he states "submuriate of zinc," &c. "appeared alkaline to turmeric paper." Further on, it is stated that "the effect is produced principally by the *acid* present." How can this be, in the case of subacetate of lead? Does this too become a *persalt?* But he afterwards remarks that "the whole substance must act."

Now the truth is, that all that was done before my experiments, consisted in Desfosses showing that boracic &c. acids reddened turmeric paper; and Mr. F. *thinks* that South had found that subacetate of lead reddened turmeric paper. It may be so, for any thing I know to the contrary; but *I* have never met with the detail. From all this however it was merely supposable that turmeric, as turmeric, was susceptible of this change by the action of boracic acid, &c. But I have adverted not only to turmeric, but to other vegetable colours, as Syrup of Violets, Tincture of Cabbage, Columbine, &c.

In the "Transactions of the Royal Society of London," Mr. Smithson has ingeniously supposed that the transit from *red to blue*, exhibited on the rupture of the leaves of the red cabbage, &c. is owing to the *escape of carbonic acid gas*. This however is most certainly *not the case*. I am induced to ascribe it to the loss of a portion of its *latent caloric*, for the following reasons: If the vessels be ruptured in a *heated* atmosphere, the colour *continues red*. If the blue infusion be heated, it becomes red, which is completely fatal to Mr. Smithson's conclusion. The Tincture of Cabbage will even change to red in a tube *hermetically sealed*.

Simply *heat* blue litmus paper or unsized paper tinged with Tincture of Cabbage, and such will become *red;* and the *original blue colour will be restored* on dipping the paper into *diluted alkali*.

Let paper stained with Tincture of Cabbage, and subsequently *reddened with acetous acid*, be exposed to the effects of *radiation* in an unclouded nocturnal sky, and the *blue colour will return*.

I found that a portion of *ice* well washed with distilled water, turned the Tincture of *Red Beet* to a permanent *dark brown*.

In fact, I have found that *heat*, in *numerous cases*, has superinduced a change of colour similar to that effected by *acids*, and *cold* similar to those changes connected with *alkaline* action.

These facts clearly prove that we have been completely wrong in the lines of demarcation we have drawn around alkalis and acids in their relations to vegetable colours as characteristic of their respective features; and I cannot doubt that a more subtile and refined chemistry, the result of an extended stage of our beautiful science, will reveal to us many more blots;—blots that the hand of experiment only can deface.

With every respect I have the honour to be,
Gentlemen,
Your much obliged and very obedient humble servant,
J. Murray.

Aug. 2, 1822.

I believe (for in truth I very seldom read over what I have written, certainly never take or retain a copy of my communications) I omitted to note, "on the relation of acids and alkalis to vegetable colours," that the *superacetate of lead* turns the Syrup of Violets *green*, the same change being superinduced, as in the case of *subacetate of lead;*—there is therefore no necessity for ringing changes on that solitary string.

It may be interesting to mention, in reference to the *siliceous* calculus I have described to you, that the *urine* of the individual (who voided the fragments of which *silica* formed a constituent part) after a repose of nine days had deposited (spontaneously) a *white sediment* with *mucus* superimposed, and much *urea* beautifully crystalline.

At page 332 of Mr. Children's very excellent translation of Berzelius on the blowpipe (and which adds to the high claims Mr. Children, as an accomplished chemist, has on our respect and gratitude) "siliceous calculi" are described as leaving *per se* "an infusible, sometimes scoriaceous ash, which fuses with a small quantity of soda, slowly and with effervescence, into a more or less transparent glass globule."

It is respectfully submitted, as a *query*, whether the following, page 330, be sufficiently explicit to prevent the obtrusion of error?—"The slight ammoniacal odour which potassa develops with almost all animal substances has nothing to do with this," viz. that evolved in the case of calculi composed of *urate of ammonia*. I always am, gentlemen,

Your obliged and obedient servant,

J. Murray.

XXIX. *New Demonstrations of the Method invented by* Budan, *and improved by others, of extracting the Roots of Equations. By Mr.* Peter Nicholson[*].

Theorem.

If $Ax^{n-1}+Bx^{n-2}+Cx^{n-3}+\&c=Pv^{n-1}+Qv^{n-2}+Rv^{n-3}+\&c.$

and $Ax^{n}+Bx^{n-1}+Cx^{n-2}+\&c=P'v^{n}+Q'v^{n-1}+R'v^{n-2}+\&c.$

and if $x-e=v$ or $x=v+e$, then will the coefficient of the nth term of the second side of the second equation be equal to the product of the coefficient of the $(n-1)$th term of the second side of the first equation, and the given quantity e plus the coefficient of the nth term next following.

That is, $P'=P$, $Q'=Pe+Q$, $R'=Qe+R$, $S'=Re+S$ &c.

Demonstration.

For by hypothesis,

$$Ax^{n-1}+Bx^{n-2}+Cx^{n-3}+\&c.=Pv^{n-1}+Qv^{n-2}+Rv^{n-3}+\&c.$$

Multiply the first side of this equation by x, and the second side by its equal $v+e$, and we have the equation

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[*] Communicated by the Author.

$$Ax^n + Bx^{n-1} + Cx^{n-2} + \&c. = \begin{cases} Pv^n + Qv^{n-1} + Rv^{n-2} + \&c. \\ \qquad + Pev^{n-1} + Qev^{n-2} + \&c. \end{cases}$$

Now this is the second equation of condition; therefore $P' = P$, $Q' = Pe + Q$, $R' = Qe + R$, $S' = Re + S$, and so on; whence the proposition is manifest.

Corol. 1.—Hence the whole function $P'v^n + Q'v^{n-1} + R'v^{n-1}$ + &c. may easily be derived from the given function

$$Ax^n + Bx^{n-1} + Cx^{n-2} + \&c.$$

Corol. 2.—Hence because $P' = P$, P in the first derived function must be equal to A, as will appear by the following table:

$$\begin{aligned} A &= A \\ Ax + B &= Av + {}_1B \\ Ax^2 + Bx + C &= Av^2 + {}_2Bv + {}_2C \\ Ax^3 + Bx^2 + Cx + D &= Av^3 + {}_3Bv^2 + {}_3Cv + {}_3D \\ \&c. \quad & \quad \&c. \end{aligned}$$

Whence by the property shown in the proposition demonstrated, the values of the coefficients ${}_1B$, ${}_2B$, ${}_3B$, &c. of the second term respectively of the first, second, third, &c. derived function, the values ${}_2C$, ${}_3C$, &c. of the third term respectively of the second, third, &c. derived function, and so on, may easily be obtained from one another, as will appear by the following table:

$$\begin{array}{c|c|c|c} {}_1B = Ae + B & & & \\ {}_2B = Ae + {}_1B & {}_2C = {}_1Be + C & & \\ {}_3B = Ae + {}_2B & {}_3C = {}_2Be + {}_2C & {}_3D = {}_2Ce + D & \\ \&c. & \&c. & \&c. & \&c. \end{array}$$

And generally

$${}_nB = Ae + {}_{n-1}B,\ {}_nC = {}_{n-1}Be + {}_{n-1}C,\ {}_nD = {}_{n-1}Ce + {}_{n-1}D, \&c.$$

PROBLEM.—To transform any rational function of an unknown quantity into another in which the unknown quantity shall be greater or less than that in the proposed function by a given difference.

To perform this problem, it would only be necessary to attend to the table at the end of the demonstration; but to give greater facility in the execution I shall endeavour to express the table in words.

Let *e* represent the quantity to be added or taken away from the unknown quantity in the proposed function, as in the proposition.

Place the coefficients and absolute numbers A, B, C, &c. of the given function, with their proper signs, in a line, and in the same order as they stand in that given function.

Add the product Ae of the first coefficient A, and the quantity e to the second coefficient B, and write the sum a' under the second coefficient.

Multiply this sum a' by the quantity e, and add the product to the third coefficient C, and write the sum a'' under C, the third coefficient in a second line.

Proceed in this manner until the remaining coefficients and the absolute number have been used, so that each respective sum may fall in a line below that last found under the preceding coefficient.

To complete the columns under the coefficient of the second term, add the product of the first coefficient A and the quantity e to the number a' found under the second coefficient, and write the sum b' under this number in a line with the number a'' under the third coefficient.

Proceed with every succeeding quantity in the second column in the same manner, until as many quantities or numbers have been found as the exponent of the highest power contains units.

In general, when any two adjacent numbers in the same line or horizontal row are found, by adding the product of the first number and the quantity e to the second number, the sum is the next number under the second of these two given numbers.

Then, when all the columns are completed, write the coefficient of the first term of the given function before the number at the bottom of the column under the coefficient of second term of the proposed equation: then the lowermost horizontal row will contain the coefficients of the transformed function.

The form of operation as now directed for the quadratic function Ax^2+Bx+C is as follows:

$$\begin{array}{rl} x, & \underline{A + B + C}\,(e \\ & \underline{\quad + a'} \\ x-e, & \underline{A + b' + a''} \end{array}$$

So that the function Ax^2+Bx+C is transformed to $A(x-e)^2+b'(x-e)\,t\,a''$.

The form of operation for the cubic function Ax^3+Bx^2+Cx+D is as follows:

$$\begin{array}{l} \underline{A + B + C + D}\,(e \\ \quad + a' \\ \quad + b' + a'' \\ \underline{A + c' + b'' + a'''.} \end{array}$$

And thus the cubic function Ax^3+Bx^2+Cx+D is transformed to $A(x-e)^3+C(x-e)^2+b''(x-e)+a'''$.

And so on for the higher powers.

Example.—Given the function $4x^3+15x^2-6x+15$ to transform it into another in $x-3$.

Operation.

$$\begin{array}{rrrr}
4 & +15 & -6 & +15(3 \\ \hline
 & +27 & & \\
 & 39 & +75 & \\
4 & +51 & +192 & +240. \\ \hline
\end{array}$$

Therefore,

$$4x^3+15x^2-6x=4(x-3)^3+51(x-3)^2+192(x-3)+240.$$

Roots of Equations.

By the application of this method of transforming algebraic functions, we may easily discover the initial values of all the roots of any given equation, by attending to the following property, first given by Descartes.

"Every equation (if impossible roots be allowed to make up the number) exhibits as many changes of signs as it has positive roots, and as many continuations of the same sign as it has negative roots."

Therefore, if, in diminishing the root of an equation by n of any denomination of figures, and if the transformed equation in $x-n$ has the same number of changes of signs as the original equation in x, but if in diminishing the root of the transformed equation in $x-n$ by unity, a certain number of changes of signs disappear; then n will be the initial value of as many roots of the original equation as the number of changes which are lost in the last transformation in $x-n-1$.

Example.—Transform the equation $x^4-5x^3+3x^2-9x+4=0$ by diminishing its root by unity, the root of the transformed equation by 3, and the second transformed equation by unity.

$$\begin{array}{lrrrrr}
x, & 1 & -5 & +3 & -9 & +4(1 \\ \hline
 & & -4 & & & \\
 & & -3 & -1 & & \\
 & & -2 & -4 & -10 & \\
x-1, & 1 & -1 & -6 & -14 & -6(3 \\ \hline
 & & +2 & & & \\
 & & +5 & \mp 0 & & \\
 & & +8 & +15 & -14 & \\
x-4, & 1 & +11 & +39 & +31 & -48(1 \\ \hline
 & & +12 & & & \\
 & & +13 & +51 & & \\
 & & +14 & +64 & +82 & \\
x-5, & 1 & +15 & +78 & +146 & +34. \\ \hline
\end{array}$$

In passing from the equation in x to that in $x-1$ three changes of signs have disappeared; therefore between 0 and 1, there is at least one root, and may be three, and one change of signs remain. In passing from the equation in $x-1$ to that in $x-4$ the equation in $x-4$ has still the same number of changes of signs as that in $x-1$, viz. one change; but in passing from the equation in $x-4$ to that in $x-5$, that change of signs has disappeared; we therefore conclude that one or three roots of the original equation in x lie between 0 and 1, and that another of the roots lies between 4 and 5.

Therefore, if all the three first roots are real, zero will occupy the unit's place of each of these roots, and the first figure of the other root is 4.

As an exercise of making a research of the roots in the place of tenths, the following example is given.

Example 1.—Let it be required to find the initial values of the three roots of the equation $x^4-5x^3+3x^2-9x+4=0$, which lie between zero and unity.

$$\begin{array}{ll}
x, & \underline{1-\;5+\;3-\;9+\;4}\,(0{\cdot}1 \\
 & -49 \\
 & -48+251 \\
 & -47+203-8749 \\
x-0{\cdot}1, & \underline{1-46+156-8546+31251}\,(0{\cdot}1 \\
 & -45 \\
 & -44+111 \\
 & -43+\;67-8435 \\
x-0{\cdot}2, & \underline{1-42+\;24-8368+22811}\,(0{\cdot}1 \\
 & -41 \\
 & -40-\;17 \\
 & -39-\;57-8385 \\
x-0{\cdot}3, & \underline{1-38-\;96-8442+14431}\,(0{\cdot}1 \\
 & -37 \\
 & -36-133 \\
 & -35-169-8575 \\
x-0{\cdot}4, & \underline{1-34-204-8744+5856}\,(0{\cdot}1 \\
 & -33 \\
 & -32-237 \\
 & -31-269-8981 \\
x-0{\cdot}5, & \underline{1-30-300-9250-3125.}
\end{array}$$

In passing from the original equation in x to the equation in $x-0{\cdot}2$ four changes of signs still remain; but in passing from the equation in $x-0{\cdot}2$ to that in $x-0{\cdot}3$ there are only two changes of signs; therefore two roots, real or imaginary, are contained between 0·2 and 0·3: therefore if these two roots are real, the initial figure of each of them is 2. Again, in passing from the equation in $x-0{\cdot}3$ to that in $x-0{\cdot}4$, the two changes of signs still remain; but in passing from the equation in $x-0{\cdot}4$ to that in $x-0{\cdot}5$ one of these changes has disappeared;

peared; therefore the initial figure of another real root is 0·4. One method of discovering impossible roots is as follows; viz.

When any coefficient of an equation becomes zero, and if the signs of the adjacent coefficients on each side are like, that equation contains at least two impossible roots.

Another rule which I can demonstrate, is as follows:

When any column of coefficients has a minimum, and if two changes of signs take place in the transformed equations, at that minimum the original equation has two impossible roots. We shall find the fourth coefficients of the equations in $x-0·1$, $x-0·2$, and $x-0·3$ to be respectively $-8·546$, $-8·368$, and $-8·442$, which show a minimum and consequently two impossible roots.

Extraction of the Roots of Equations.

If we find the initial figure or figures of a root, we may extract the root of the original equation in x by extracting the root of the transformed equation, thus: Divide the absolute number, annexing a cipher, by the coefficient of the single power, and the first figure of the quotient is the next figure of the root of the original equation to the initial figure or figures already found.

Proceed in this manner from one denomination to another, until as many figures are found as may be thought necessary.

Example.—Find that root of the equation $x^4-5x^3+3x^2-9x+4=0$ of which the initial figure has been found to be 0·4, the corresponding transformed equation being

$$(x-4)^4-34(x-4)^3-204(x-4)^2-8744(x-4)+5856=0.$$

Now by dividing 58560 by 8744 we obtain ·6; and here the whole are regarded as if they had been integers and therefore the next figure tenths.

Operation.

1	−34	−204	−8744	+5856 (·658
	−334			
	−328	−22404		
	−322	−24372	−8878424	
	−316	−26304	−9024656	+5289456
	−3155			
	−3150	−2646175		
	−3145	−2661925	−9037886875	
	−3140	−2677650	−9051196500	+7705125625
	−31392			
	−31384	−268016136		
	−31376	−268267208	−9053340629088	
	−31368	−268518216	−9055486766752	+4624531217296.

When the entire and correct root of the original equation in x, as far as the number of figures go, is $x=·4658$.

XXX. *On the Electrical Phænomena exhibited in Vacuo.* By Sir HUMPHRY DAVY, *Bart. P.R.S.*[*]

THE production of heat and light by electrical discharges; the manner in which chemical attractions are produced, destroyed, or modified by changes in the electrical states of bodies; and the late important discovery of the connexion of magnetism with electricity, have opened an extensive field of inquiry in physical science, and have rendered investigations concerning the nature of electricity and the laws by which it is governed, and the properties that it communicates to bodies, much more interesting than at any former period of the history of philosophy.

Is electricity a subtile elastic fluid? or are electrical effects merely the exhibition of the attractive powers of the particles of bodies? Are heat and light elements of electricity, or merely the effects of its action? Is magnetism identical with electricity, or an independent agent, put into motion or activity by electricity?—Queries of this kind might be considerably multiplied, and stated in more precise and various forms: the solution of them, it must be allowed, is of the highest importance; and though some persons have undertaken to answer them in the most positive manner, yet there are, I believe, few sagacious reasoners, who think that our present data are sufficient to enable us to decide on such very abstruse and difficult parts of corpuscular philosophy.

It appeared to me an object of considerable moment, and one intimately connected with all these queries, *the relations of electricity to space, as nearly void of matter as it can be made on the surface of the earth*; and, in consequence, I undertook some experiments on the subject.

It is well known to the Fellows of this Society who have considered the subject of electricity, that Mr. Walsh believed that the electrical light was not producible in a perfect torricellian vacuum; and that Mr. Morgan drew the same inference from his researches; and likewise concluded that such a vacuum prevented the charging of coated glass.—Now it is well known, that in the most perfect vacuum that can be made in the torricellian tube, vapour of mercury, though of extremely small density, exists; I could not help, therefore, entertaining a doubt as to the perfect accuracy of these results, and I resolved not only to examine them experimentally, but likewise, by using a comparatively fixed metal in fusion for making the vacuum, to exclude, as far as was possible, the presence of any volatile matter.

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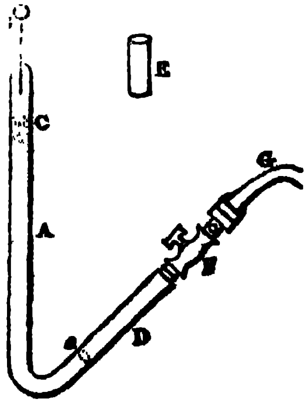
[*] From the Transactions of the Philosophical Society for 1822, Part I.

The



The apparatus that I employed was extremely simple, (see the figure,) and consisted of a curved glass tube with one leg closed and longer than the other. In this closed leg a wire of platinum was hermetically cemented, for the purpose of transmitting the electricity: or to ascertain the power of the vacuum to receive a charge, it was coated with foil of tin or platinum. The open end, when the closed leg had been filled with mercury or any other metal, was exhausted either by being placed under the receiver, or connected with the stop-cock of an excellent air-pump; and in some cases, to ensure greater accuracy, the exhaustion was made after the tube and apparatus had been filled with hydrogene*.

Operating in this way, it was easy to procure a vacuum either of a large or small size; for the rarefied air or gas could be made to balance a column of fluid metal of any length, from 20 inches to the 20th of an inch, and by using only a small quantity of metal, it could be more easily purged of air.

I shall first mention the results I obtained with quicksilver. I found that by using recently distilled quicksilver in the tubes, and boiling it *in vacuo* six or seven times from the top to the bottom, and from the bottom to the top, making it vibrate repeatedly by striking it with a small piece of wood, a column was obtained in the tube free from the smallest particle of air; but a phænomenon occurred, in discovering the cause of which I had a great deal of trouble. When I used a short tube of four or five inches long only, I found, that after continued boiling and much agitation of the mercury, though there was no appearance of elastic matter, when the mercury adhered strongly in the upper part of the tube, yet that, after electrization, or even on suffering the mercury to pass slowly back into the closed part, a minute globular space sometimes appeared: I thought at first that this was air, which, though so highly rarefied as it must have been by the exhaustion, adhered to the mercury; and I endeavoured by long boiling the mercury in an exhausted *double* syphon, and making the vacuum in one of the curves, to prevent entirely the presence of air: but the phænomenon always occurred when there was no strong adhesion of the mercury to the glass. This, and another circumstance, namely, that when the leg in which the torricellian vacuum was made was 15 or 16 inches long, the phænomenon was very rarely perceptible, and always disappeared when the tube was inverted, and the mercury made to strike the top with some force, led me to conclude that the minute space was really filled with the vapour of mercury; the attraction of the particles of the fluid mercury for each other preventing their actual contact with the glass, except when this contact was forcibly made by mechanical means; and I soon proved that this was the case: for by causing the mercury, when its column was short, to descend into the more perfect from the less perfect vacuum, with more or less velocity, I could make the space more or less, or cause its disappearance altogether, in which last case the cohesion between the mercury and the glass was always extremely strong.

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* The figure will best explain the form of the apparatus.

I found that in all cases when the mercurial vacuum was perfect, it was permeable to electricity, and was rendered luminous by either the common spark, or the shock from a Leyden jar, and the coated glass surrounding it became charged; but the degree of intensity of these phænomena depended upon the temperature: when the tube was very hot, the electric light appeared in the vapour of a bright green colour, and of great density; as the temperature diminished, it lost its vividness; and when it was artificially cooled to 20° below zero of Fahrenheit, it was so faint as to require considerable darkness to be perceptible.

The charge likewise communicated to the tin or platinum foil was higher the higher the temperature; which, like the other phænomenon, must depend upon the different density of the vapour of mercury; and at 0° Fahrenheit it was very feeble indeed.

A very beautiful phænomenon occurred in boiling the mercury in the exhausted tube, which showed the great brilliancy of the electrical light in pure dense vapour of mercury. In the formation and condensation of the globules of mercurial vapour, the electricity produced by the friction of the mercury against the glass, was discharged through the vapour with sparks so bright as to be visible in day light.

In all cases when the minutest quantity of rare air was introduced into the mercurial vacuum, the colour of the light produced by the passage of the electricity changed from green to sea green; and, by increasing the quantity, to blue and purple; and when the temperature was low, the vacuum became a much better conductor.

I tried to get rid of a portion of the mercurial vapour, by using a difficullty fusible amalgam of mercury and tin, which was made to crystallize by cooling in the tube; but the results were precisely the same as when pure mercury was used.

I tried to make a vacuum above the fusible alloy of bismuth; but I found it so liable to oxidate and dirt the tube, that I soon renounced further attempts of this kind.

On a vacuum above fused tin I made a number of experiments; and by using freshly cut pieces of grain tin, and fusing them in a tube made void after being filled with hydrogen, and by long continued heat and agitation, I had a column of fused tin which appeared entirely free from gas: yet the vacuum made above this, exhibited the same phænomena as the mercurial vacuum. At temperatures below 0°, the light was yellow, and of the palest phosphorescent kind, requiring almost absolute darkness to be perceived; and it was not perceptibly increased by heat.

I made two experiments on electrical and magnetic repulsions and attractions in the mercurial vacuum, by attaching to the platinum wire two fine wires in one case of platinum, in the other of steel, terminated by minute spherules of the same metals; I found that they repelled each other when the wire was electrified in the most perfect mercurial vacuum, as they would have done in usual cases; and the steel globules were as obedient to the magnet as in the air; which last result it was easy to anticipate.

In some of the first of these experiments, I used a wire for connecting the metal with the stop-cock; but latterly, the rarefied air or gas was the only chain of communication: and this circumstance enabled me to ascertain that the feebleness of the light in the more perfect vacuum was not owing merely to a smaller quantity of electricity passing through it; for the same discharge which produced a faint green light in the upper part of the tube, produced a bright purple light in the lower part, and a strong spark in the atmosphere.

The boiling point of pure olive oil is not much below that of mercury; and the butter or chloride of antimony (antimonane) boils at about 388° Fahrenheit. I tried both these substances in the vacuum, and found, as might be expected, that the light produced by the electricity passing through the vapour of the chloride was much more brilliant than that produced by it in passing through the vapour of the oil; and in the last it was more brilliant than in the vapour of mercury at common temperatures; the lights were of different colours, being of a pure white in the vapour of the chloride, and of a red, inclined to purple, in that of the oil; and in both cases permanent elastic fluid was produced by its transmission.

The law of the diminution of the density of vapours by diminution of temperature, has not been accurately ascertained; but I have no doubt, from the experiments of Mr. Dalton, and some I have made myself, that it is represented by a geometrical progression; the decrements of temperature being in arithmetical progression; and in three pure fluids that I operated upon*, the ratio seemed nearly uniform for the same number of degrees below the boiling point; and (taking intervals of 20 degrees of temperature) ·369416. Upon this datum, and considering the boiling points of mercury to be 600°, that of oil 540°, that of the chloride of antimony 340°, and that of tin 5000°, all above 52°, and the elastic force of vapour of water at this temperature to be equal to raise by its pressure about ·45 parts of an inch of mercury; the relative strengths of vapour will be, for mercury 000015615, for oil 0016819, for chloride of antimony 01692, and for tin 37015, preceded by 48 zeros†.

It is not known whether the vapour from solids follows a similar law of progression as that from fluids, and these numbers are only given to show how minute the quantity of matter must be in vapours where its effects are distinct upon electrical phænomena; and how much more minute it must be in the case of mercury artificially cooled; and almost beyond imagination so in vapours from substances requiring very elevated temperatures for their ebullition.

I made some comparative experiments to ascertain whether below the freezing point of water, the diminution of the temperature of the torricellian vacuum diminished its power of transmitting electricity, or of being rendered luminous by it. To about 20° this appeared to be the case; but between 20° above and 20° below zero, the lowest temperature I could produce by pounded ice and muriate of lime, it seemed stationary; and as well as I could determine, the electrical phænomena were nearly of the same intensity as those produced in the vacuum above tin.

Unless the electrical machine was very active, no light was visible during the transmission of the electricity; but that this transmission took place, was evident from the luminous appearance of the rarefied air in the other parts of the syphon, and from the diminution of the repulsion of the ball of the quadrant electrometer attached to the prime conductor. When the machine was in great activity, there was a pale phosphorescent light above, and a spark on the mercury below, and brilliant light in the common vacuum. A Leyden jar *weakly* charged could not be made to transmit its electricity by explosion through the cooled torricellian vacuum, but this electricity was slowly dissipated through it; and when *strongly* charged, the spark passed through nearly as much space as in common air, and with a light visible in the shade. At all temperatures below 200°, the mercurial vacuum was a much worse conductor than highly rarefied air: and when the tube containing it was included in the exhausted receiver, its temperature being about 50°, the spark passed through a distance six times greater in the Boylean than in the mercurial vacuum.

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* Water, chloride of phosphorus, and alcohol or carburet of sulphur.

† I am obliged to Charles Babbage, Esq. F.R.S. for these calculations.

It is evident from these general results that the light (and probably the heat) generated in electrical discharges depends *principally* on some properties or substances belonging to the ponderable matter through which it passes; but they prove likewise that space, where there is no appreciable quantity of this matter, is capable of exhibiting electrical phænomena; and, under this point of view, they are favourable to the idea of the phænomena of electricity being produced by a highly subtile fluid or fluids, of which the particles are repulsive, with respect to each other, and attractive of the particles of other matter. On such an abstruse question, however, there can be no demonstrative evidence. It may be assumed, as in the hypothesis of Hooke, Huygens, and Euler, that an ethereal matter, susceptible of electrical affections, fills all space; or that the positive and negative electrical states may increase the force of vapour from the substances in which they exist; and there is a fact in favour of this last idea which I have often witnessed—when the Voltaic discharge is made in the Boylean vacuum, either from platinum or charcoal, in contact with mercury, the discharging surfaces require to be brought very near in the first instance; but the electricity may be afterwards made to pass to considerable distances through the vapour generated from the mercury or charcoal by its agency; and when two surfaces of highly fixed metal, such as platinum or iron, are used, the discharge will pass only through a very small distance, and cannot be permanently kept up.

The circumstance, that the intensity of the electrical light in the mercurial vacuum diminishes as it is cooled to a certain point, when the vapour must be of almost infinitely small density, and is then stationary, seems strongly opposed to the idea, that it is owing to any *permanent* vapour emitted constantly by the mercury. The results with tin must be regarded as more equivocal; because as this substance cannot be boiled *in vacuo*, it may be always suspected to have emitted a small quantity of the rare air or gas to which it has been exposed: yet, supposing this circumstance, such gas must be at least as highly expanded as the vapour from cooled mercury, and can hardly be supposed capable of affording the dense light, which the passage of the electricity of the charged Leyden phial through the vacuum produces.

When the intense heat produced by electricity is considered, and the strong attractive powers of differently electrified surfaces, and the rapidity of the changes of state, it does not seem at all improbable, that the superficial particles of bodies, which, when detached by the repulsive power of heat, form vapour, may be likewise detached by electrical powers, and that they may produce luminous appearances in a vacuum, free from all other matter by the annihilation of their opposite electrical states.

In common cases of electrical action, the quantity of the heat generated by the annihilation of the different electrical states depends, as I stated in my last communication to the Society, upon the nature of the matter on which it acts; and in cases when electrical sparks are taken in fluids, vapour or gas is always generated; and in elastic fluids, the intensity of the light is always greater, the denser the medium. The luminous appearances therefore, it is evident from all the statements, must be considered as secondary; whilst the uniform exertions of electrical attractions and repulsions, under all circumstances, in rare and dense media and in vacuo, and with respect to solids, fluids, and gases, point them out (whether they be specific affections of a subtile imponderable fluid, or peculiar properties of matter) as primary and invariable electrical phænomena.

I have mentioned in the last page the suspicion, that melted tin may contain air. I shall conclude this paper by stating the grounds of this suspicion, and noticing a circumstance which appears to be of considerable importance, both in relation to the construction of barometers and thermometers, and to the analysis of gaseous bodies. Recently distilled mercury that has been afterwards boiled and cooled in the atmosphere, and which presents a perfectly smooth surface in a barometer tube, emits air when strongly heated in vacuo, and that in quantities sufficient to cover the whole interior of the tube with globules; and on keeping the stop-cock of one of the tubes used in the experiments on the mercurial vacuum open for some hours, it was found that the lower stratum of mercury had imbibed air, for when heated in vacuo, it emitted it distinctly from a space of a quarter of an inch of the column: smaller quantities were disengaged from the next part of the column; and its production ceased at about an inch high in the tube. There is great reason to believe, that this air exists in mercury in the same invisible state as in water, that is, distributed through its pores; and the fact shows the necessity of long boiling the mercury in barometer and thermometer tubes, and the propriety of exposing as small a surface of the mercury as possible to the air. It may explain, likewise, the difference of the heights of the mercury in different barometers; and seems to indicate the propriety of boiling the mercury in these instruments after a certain lapse of time.

Explanation of the Figure.

A. The tube, of the usual diameter.

B. The wire for communicating electricity.

E. A small cylinder of metallic foil, to place as a cap on tubes not having the wire *b*, to make a coated surface.

C. The surface of the quicksilver, or fused tin.

D. The part of the tube to be exhausted by the stop-cock F, after being filled by means of the same stop-cock, when necessary, with hydrogen.

G. The moveable tube connected with the air-pump.

It is evident, that by introducing more mercury, the leg *d* may be filled with mercury, and the stop-cock closed upon it, so as to leave only a torricellian vacuum in the tube, in which the mercury may be boiled. I have found that the experiment tried in this way, offers no difference of result.

XXXI. On Mr. John Moore *Junior's* "*Reply.*"

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—I repeat that I see no necessity whatever for *republishing* on the part of Mr. John Moore junior that which I had already done. The *public* have nothing to do with private intromissions. It was priority of publication which secured for Sir H. Davy the imperishable honours attached to his beautiful invention. It is this which is the standard of appeal in science and art.

I have given my *reasons* for rejecting the cumbrous and troublesome modification obtruded. These reasons remain inviolate.

The "Reply" is a mere tissue of *questions:* FOURTEEN marks of interrogation are interspersed! A very *convenient* mode of "reply," it must needs be confessed. For instance, I am asked, "How is it that the individual becomes reanimated?" This is introduced as a species of *climax* to a most *disingenuous* (I shall not term it wilful or malignant) perversion of my language. Mr. Moore junior says, "Mr. Murray has stated that the air undergoes no change whatever!!" Whereas my words are, "the air undergoes no change whatever *until natural respiration returns.*

There is not a word more that calls for notice; the remainder of these interrogatories being awkwardly apologetic, rather than any thing else.

I have the honour to be, gentlemen,
Your much obliged and very obedient humble servant,
August 2, 1822. J. Murray.

XXXII. *On the Cause of a singular partial Failure in a Crop of Turnips. By Mr.* Samuel Taylor, *of Bungay.*

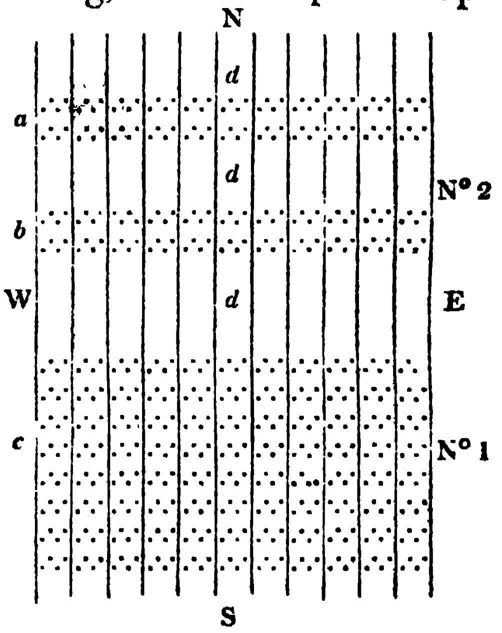
To the Editors of the Philosophical Magazine and Journal.

Gentlemen, — As I find you do not exclude from your pages any information, however humble, on subjects connected with the study and practice of agriculture, allow me to lay before you a singular instance of partial failure in a field of turnips, which has recently come under my observation. This field is in the hands of my friend and neighbour Mr. Stamford, of Ditchingham, near this place. It may be necessary here to remark, that it is usual, in working our fallows intended for turnips, to cross-plough the land twice, in order to get the field perfectly level previous to stetching or ridging it up for the ensuing crop. Now the ridges or stetches of this field run north and south; but the failure in the turnip crop above mentioned extends *across*, not *lengthways* of the ridges, and consequently in the direction of the *warting*.

The following sketch may give a clearer idea than mere description can convey of my meaning, and of the present appearance of the field.

a
b
c } Ploughed beginning of May: good healthy plants.

d
d
d } Left unploughed from middle of May to beginning of June: a total failure of plant.



Now what has occasioned this failure? and on what principle can we account for the narrow strips of good healthy plants *across* the field at *a* and *b*, whilst those on each side of them have totally failed? An inquiry into the mode of culture will, perhaps, explain what at first sight appears rather mysterious. It seems that the field was *warted* or cross-ploughed for the first time about the middle of April; the wheat stubble having been first *turned* in, as we call it, *i.e.* ploughed, before winter. It was again *warted* the beginning of May. The whole field was stetched up for sowing about the beginning of June (being the *fourth* earth it had received); but in consequence of the long drought, it was not sown till the 12th of July. The seed (Swedish) was put on *broadcast* in one day. The plants came to the hoe in about a month after; but for some time previous to hoeing, a partial deficiency of plant was noticed. Mr. S. was for a while at a loss to account for so singular and *regular* a failure, until reminded by his men of a circumstance which had escaped his recollection. The part of the field No. 1 lies lower, and is consequently more liable to injury from wet, than the upper part, No. 2. Having therefore finished cross-ploughing No. 1, he, knowing from the dryness of the soil that No. 2 could be ploughed at any time, took off his men and horses to some other work of more immediate importance; so that the whole of No. 2, *except a few furrows which had been ploughed* (marked *a* and *b*), lay for above a fortnight untilled. The deficiency of plant is therefore clearly attributable to this circumstance, because the whole of the remainder of the field, including the strips *a* and *b*, produced good healthy plants: but then occurs the question, How could this suspension of operations for two or three weeks occasion such extraordinary effects? The answer appears to be this: The ground not being stirred during this interval, a multitude of grubs and wireworms were thereby suffered to hatch, which the plough would otherwise have destroyed; and these have doubtless eaten the plants. This idea appears to derive confirmation from the number of rooks which have ever since continued to alight on the bare spots: on removing the surface earth of which, both grubs and wireworms are still found just buried beneath the mould.

Perhaps some of your readers better versed in these matters than I am, can give a more satisfactory solution of the above phænomenon. I merely state the facts, and endeavour to put the most reasonable interpretation on them in my power. If I am right, it would follow that it is not good to allow too long an interval between the spring ploughings; but I should be glad to know the opinion of entomologists on this subject.

In the mean time, I remain, gentlemen,

Your most obedient humble servant,

Bungay, Suffolk, Aug. 20, 1822. SAMUEL TAYLOR.

XXXIII. *On the Stars forming the Pleiades*[*].

The following catalogue contains a list of the 64 stars forming the *Pleiades* given by M. Jeaurat in the *Mémoires de l'Académie des Sciences* for 1779; and brought up to January 1, 1822, in the manner hereafter mentioned. The importance of this singular cluster of stars, at the present time, has induced me to give a separate list of them. For, it is well known that the moon is now in such a position, with respect to her nodes, as to pass over the *Pleiades* every lunation in the present and several following years: thereby affording a favorable opportunity not only for such observations, but more particularly for illustrating the theory of Cagnoli, with respect to his mode of determining the figure of the earth, by means of occultations of the fixed stars by the moon[†]. The passage of the moon across the *Pleiades* has indeed at all times attracted the attention of astronomers; as may be seen by the numerous observations recorded in the transactions of all the scientific societies, whenever the circumstance has taken place. In 1653 Kepler gave a chart of the *Pleiades*, in the supplement to his Treatise on Optics: it consisted of 6 principal stars, and 26 smaller ones. In the *Mém. de l'Acad. des Sciences* there are four charts by different authors: one by La Hire in 1693; another by Cassini and Maraldi in 1708; and another by Outhier in 1770: but the most comprehensive one is that by Jeaurat, as above mentioned, accompanied by a list of their positions. This list, however, bears several inaccuracies on the face of it: (which indeed may be readily detected on a close inspection of the original paper) and moreover the positions, when laid down on the chart, do not correspond exactly with the present relative positions in the heavens. One remarkable error pervades the whole of the original list. The right ascensions in time exceed (by $1''$ in time) the right ascensions in arc. In forming the present table, however, I have depended principally upon his list of *differences* in AR and D of the several stars from *Alcyone*[‡]: and the position of this star (for the commencement of the present year) I have deduced from Mr. Pond's catalogue of 400 stars. The mode, by which M. Jeaurat formed his catalogue, was this: He determined the position of nine of the brightest stars, by means of the mural circle at the observatory (placed there by La Hire in 1682); *those being all he could distinguish with the telescope attached to that instrument*[*]*:* and from the positions of these, thus determined, he deduced, with a telescope of 32 inches focus, the positions of the remaining fifty-five. Under these circumstances it cannot be expected that the positions are given with that degree of accuracy which distinguishes more modern observations: nevertheless they are more accurate than those of any of his predecessors. And as no observations on this remarkable cluster of stars have been made since his time (or if made, have not been recorded) I present them as the best that I can procure; under the hope that some of our present observers will turn their attention to this subject, and favour the world with a more correct and comprehensive catalogue. With a view to assist the observer, and to enable him to identify the several stars here alluded to, I have caused a chart to be engraved, with their several positions laid down, as given in the table[†]. I have not in the chart paid any attention to M. Jeaurat's magnitudes, as they are evidently erroneous: but have adopted a scale more corresponding with their present appearance. Those given by M. Jeaurat, in the table, are in most cases much too great: and it is singular that so considerable an error in their *comparative* brightness should have arisen, when most of the stars must have been in the field of the telescope at one and the same moment. Moreover, the magnitudes, represented on his chart, do not at all correspond with those mentioned in the catalogue. I ought also to notice his singular suspicion that *Pleione* has a proper motion, from east to west, round *Alcyone*, at the rate of one degree in 100 years!!! Above two-thirds of that period has elapsed since the observations of Bradley; and *Pleione* still retains its relative position[‡].

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[*] From "Astronomical Tables and Remarks for the year 1822, by Francis Baily, Esq." a work printed for private circulation only.

[†] See my translation of his Memoir on this subject.

[‡] This, it is evident, is not strictly correct, since the precession of the several stars will differ. But, it will be sufficiently so in the present case, where the original observations are subject to so much uncertainty.

In the column of synonyms, those numbers, to which the letter P is affixed, refer to Piazzi's catalogue. The letter M denotes Mayer's. The other numbers are Flamsteed's.

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[*] And yet No. 9 (a star of the 8th magnitude) was one of these: although there are several others of the 4th and 5th magnitude recorded in the list!!!

[†] This chart, Mr. Baily has kindly permitted us to print, for the present Number of this Journal.—Edit.

[‡] The presumed *proper motion* of many other stars would (I conceive) if nicely examined, vanish in a similar manner. Good observations compared with bad ones, or even with good ones improperly or *unequally* reduced, are very inadequate tests of the proper motion of a star: and I fear the major part are in this situation. Many stars, however, have unquestionably a motion, the principles of which cannot be explained: and this, by general consent, is called a *proper motion*. I do not object to the term, so long as a correct idea is affixed to it.

Mean Places of the Stars Jan. 1, 1822.

Synonyms.	Jeaurat's		Right Ascension.						Declination. north.		
	No.	Mag.	in time.			in arc.					
			h	′	″	°	′	″	°	′	″
	1	5	3	33	2	53	15	30	23	48	15
	2	5		33	17		19	15		55	35(*)
	3	7		33	46		26	30		33	5(†)
	4	6		33	49		27	15		45	5
	5	7		33	50		27	30		49	5
16. g.*Cœleno*	6	3		34	12		33	0		41	48
17. b.*Electra*	7	3		34	17		34	15		32	28
	8	6		34	31		37	45		40	25
18. n.(P.131.)	9	8		34	32		38	0	24	14	18(‡)
19. c.*Taygeta*	10	3		34	36		39	0	23	53	40
	11	5		34	52		43	0		28	17
	12	6		34	52		43	0		53	35
(P.135.)	13	5		35	1		45	15		46	35
20. c.*Maia*	14	3		35	13		48	15		48	12
	15	5		35	16		49	0		28	17
21. k.*Asterope*¹	16	4		35	17		49	15		59	35
22. l.*Asterope*²	17	4		35	25		51	15		58	35
	18	7		35	35		53	45		37	55
	19	7		35	38		54	30		37	35
23. d.*Merope*	20	3		35	45		56	15		23	7
	21	9		35	46		56	30		32	35
	22	5		35	52		58	0		41	35
	23	5		36	6	54	1	30	22	59	35
	24	6		36	6		1	30	23	30	37
(P. 147.)	25	4		36	26		6	30		57	35
	26	5		36	26		6	30	24	15	5
	27	5		36	32		8	0	23	42	5
	28	5		36	28		7	0		24	45(§)
	29	8		36	42		10	30		34	35
24. p. (P.150.)	30	4		36	43		10	45		33	35(‖)

* Lalande makes the declination of this star 4′ more northerly.

† There is a difference of 4′ in the declination of this star, between M. Jeaurat's catalogue and his chart.

‡ Nearly in a straight line between *Taygeta* and 18.n, and about half way between them, No. 117 of Mayer's catalogue should be seen. But I have not yet been able to discover it. If we presume an error of 10′ in the declination, it will correspond with No. 9, which is called *m* by Bradley and Piazzi. Mayer's star, here alluded to, is No. 116 in Mr. Vince's catalogue. But nearly the whole of the numbers in that catalogue are wrong. Mayer's catalogue contains 998 stars; whereas Mr. Vince has only 992. He has omitted eight; and inserted two which are not to be found in Mayer's catalogue.

§ I suspect some error in the Æ of this star.

‖ Piazzi says that this star has two others near it, to the northward: one preceding it by 3″,5; and the other by 1″,5 in time. I presume No. 29 is one of them.

Synonyms.	Jeaurat's		Right Ascension.						Declination. north.		
	No.	Mag.	in time.			in arc.					
			h	′	″	°	′	″	°	′	″
	31	8	3	36	43	54	10	45	23	9	17
	32	5		36	50		12	30	24	4	35
	33	5		36	51		12	45		1	17
	34	[illegible]		36	51		12	45	23	12	17
	35	4		36	51		12	45		7	17
	36	4		36	53		13	15		19	17
25. η. *Alcyone.*	37	3		36	54		13	30		32	51
(P. 151.)	38	4		36	58		14	30		44	21
	39	5		36	58		14	30		5	17
	40	5		37	10		17	30		2	47
	41	6		37	22		20	30	22	59	47
	42	5		37	27		21	45	23	55	35(*)
	43	6		37	33		23	15		46	35
(P. 153.)	44	4		37	46		26	30		48	35
	45	4		38	2		30	30	22	49	35
	46	7		38	22		35	30	23	51	35
	47	5		38	24		36	0	22	41	5
	48	4		38	27		36	45	23	51	35
26. s. (P. 156.)	49	5		38	30		37	30		16	25
27. f. *Atlas.*	50	3		38	34		38	30		30	16
28. h. *Pleione*	51	4		38	35		38	45		34	59
(M. 128.)	52	6		38	40		40	0		43	35
(P. 161.)	53	4		38	46		41	30		19	25
	54	6		38	46		41	30	22	42	45
	55	5		38	49		42	15	24	1	25
	56	5		39	1		45	15	23	47	25
	57	5		39	2		45	30		38	35
	58	5		39	2		45	30	22	52	25
(P. 164.)	59	5		39	10		47	30	23	52	35
(P. 163.)	60	4		39	11		47	45		9	5
(P. 165.)	61	4		39	30		52	30		17	35
	62	4		39	34		53	30	24	1	35
(P. 171.)	63	5		39	50		57	30	23	59	35
(P. 172.)	64	4		40	18	55	4	30		24	35

* I could not discover this star in the year 1821: Lalande has noted one having the same AR, about 30′ more northerly.

XXXIV. *On*

XXXIV. *Reply to Dr.* APJOHN'S "*Additional Remarks,*" *&c. in* The Annals of Philosophy *for September* 1822. *By* JOHN HERAPATH, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

London, Cranford, Sept. 5, 1822.

DEAR SIRS, — IN the Annals for June I gave a formula for determining the specific gravity of aqueous vapours in contact with their fluids. The method of my finding this theorem, and the principles I employed, appeared to me so obvious and simple, that I could scarcely believe the formula itself could be new: and certainly did not imagine that my reasoning required extended illustration. Dr. Apjohn having however, in the present month's Annals, called the accuracy of the theorem in question, I perceive I must have miscalculated the obviousness of the principles, and therefore beg to trouble you with a demonstration of them.

Let us conceive a quantity of vapour having a specific gravity S to be confined at its proper tension τ in some vessel over its fluid at the Fahrenheit temperature F. Let us now conceive this vapour and its fluid to be gradually and equally cooled to some other temperature F′, conceiving at the same time, what is unquestionably admissible though perhaps not practically possible, that the vessel confining the vapour also gradually changes its capacity, so as always to preserve to the vapour an elasticity just equal to the tension it ought to have corresponding with its temperature; and let the ultimate tension be τ' and specific gravity S′. Then because by the commonly received principles water neither evaporates nor absorbs any of its superincumbent vapours, whilst the elasticity of the vapour equals the tension of the temperature, it follows that the vapour at F′ is, the same, and of the same weight as the vapour at F; that is, the mere contact of the water has produced no effect on the vapour beyond simple communication of temperature. But equal changes of temperature being similarly and under similar circumstances communicated, must, as far as I can perceive, have the same effects from whatever bodies the communication comes. The change therefore which has been made in the volume, and consequently in the specific gravity of the vapour in contact with its fluid, by the diminution of temperature, is just equivalent to that which would have been effected by an equal diminution, if the vapour had not been connected with its fluid. But when vapours are not connected with their fluids, our philosophers tell us that their changes are, *cæteris paribus*, the same as those of the gases. This established, the truth of the expression $S' = S. \frac{\tau'}{\tau} . \frac{F+448}{F'+448}$, which I have given, easily follows from what Dr. Apjohn has himself admitted; for, it being allowed to be true in gases, it must, from what I have shown, be equally true in vapours over their fluids.

In other ways the proof of this theorem might be easily derived, but I hope, what I have said will be sufficient to convince Dr. A. that the absurdity of making one constant vary as another constant, entered into no part of my communication.

Many philosophers appear to have confused themselves by not attending to the manner in which the elasticity of vapours in juxtaposition with their fluids increases; and hence they have drawn an unwarrantable line of distinction between vapours and gases. They imagine that the rapid increase of elasticity in vapours, is to be attributed to the increased action alone, which they conceive the heat gives to the vaporous particles; whereas, if they had only considered that the specific gravity increases with the elasticity, they would have seen that it is not merely to the augmented action due to the heat, but to this, and the increased number of the vaporous particles conjointly, that the great rise of elasticity is owing. By the latter part of his paper Dr. Apjohn seems to have yielded to the same erroneous ideas, and has by this means near the end of his communication involved himself in an error a little too obvious, but which I willingly omit to notice out of respect to the candour he has displayed. The same feeling induces me to pass over one or two other things in his paper, which appear to me open to comment.

Though I have not the shade of a doubt concerning the truth of the formula in question, yet I perfectly agree with Dr. Apjohn that experimental proof is desirable; and hence I should be happy to see such an object effected. But in making these experiments I conceive something of attention, beyond what is needful to secure experimental accuracy, is necessary. For instance, if we set out from Gay Lussac's determination of the specific gravity at 212°, which Dr. A. seems inclined to recommend, we should be satisfied whether this specific gravity was determined from the pressure due to the tension of vapour at 212°, or from the pressure due to ebullition at 212°; for these pressures are not equal, as philosophers have commonly imagined, but sensibly different, as I have shown pages 441 and 442, Annals for December 1821, and page 27 Annals for January 1822; the former being about the 1-7th of an inch greater than the latter. Again, it should be observed that the formula I have given would bring out results conformable to the indications of an air thermometer. This is of little importance in temperatures beneath 212°; but in very high ranges, in which at least one experiment ought to be made, it would be of vital consequence.

I am, dear sirs,
Your obedient humble servant,
J. Herapath.

XXXV. *On simultaneous Thunder-Storms.* By Dr. T. Forster.

To the Editors of the Philosophical Magazine and Journal.

Hartwell, Sept. 3, 1822.

Gentlemen,—I have already stated my opinion that the formation of thunder-storms, and other electrical phænomena, frequently took place simultaneously in very distant parts of the atmosphere. I have lately had an opportunity of confirming this opinion, and of witnessing several of the most violent instances of storms which have ever been recorded in the memory of the inhabitants of those districts where they occurred; and of comparing them with similar phænomena in distant parts of Europe.

On Monday evening, July the 29th, while travelling from Gex to Nyon, on the Lake of Geneva, we were overtaken by one of the most violent storms I ever remember to have seen: it formed very rapidly, for at half-past four o'clock the atmosphere was clear. I observed however that on descending Mount Jura, the Alps (which form a beautiful back-ground to the view of the lake below) were intersected with clouds: the *cirrostratus* seemed to unite the summits of the range of mountains by forming long lines of cloud stretched across from one hill to another, while the more elevated ground of Mont Blanc was involved in *cumulostratus.* The air very suddenly became totally obscure, and before 5 o'clock a violent shower of rain and hail came down in torrents, accompanied by thunder and lightning: it cleared off very suddenly, but as quickly returned again; and for the space of nearly an hour there was scarcely an interval of two minutes between the most vivid flashes of lightning. I distinctly noticed the two sorts mentioned by M. Van Mons; namely, the *fulguration* followed quickly by a short and loud clap; and the *fulmination* followed at a longer interval by rolling thunder. The fulguration or forked lightning darted into the lake from several distant portions of cloud at once, and was at times of a bright blue colour. By 8 o'clock the storm subsided: on the following day, July 30th, after a bright morning, a still more violent storm occurred very suddenly in the neighbourhood of Lausanne: its formation was prodigiously rapid, so that in five minutes after bright sunshine, a torrent of rain and hail fell, which desolated many of the vineyards, and the thunder and lightning scarcely ceased during the space of several following hours. In travelling from Lausanne to Vevai, we were forced to alight from the carriage, and take refuge in a house by the road side. At Belle Airo near Vevai, the residence of M. Bart. Huber, the hail is said to have descended in stones above an inch in diameter, and to have been so destructive that scarcely one entire bunch of grapes remained on the vines after it was over. I heard of a few persons being killed and part of a house destroyed in the neighbourhood. As I travelled home along the Rhine, by way of Bâle and Strasbourg, into Holland, I found, by making inquiries, that contemporaneous storms of similar violence had been witnessed throughout a most extensive tract of country, both in France and Germany. At the Hague three men were killed, and a fourth was killed while travelling on the road towards Haerlem. At Dunkirk the lofty tower of the church was struck, and the sentinel placed at the top was attacked by the lightning and rendered senseless for some time, though he eventually recovered. I heard also of several flocks of sheep and other cattle being destroyed by the electric fluid. At Bridgenorth in England, on Sunday the 28th of July (being the day previous to the great storm at Nyon), the lightning did considerable mischief—killed one person, and a great many sheep; and I have received several similar accounts from other parts; thus proving—not only a disposition in the air to produce thunder-storms at very distant places, but proving also that they occurred in distant places with the same violent and mischievous character.

I shall be obliged by any accounts of the said storms made by your correspondents in different places, and of the state of the instruments of meteorology at the time, and communicated through your Journal: and I have the honour to remain

Your most obedient
T. FORSTER.

P.S. The thermometer at Lausanne on the 30th of July stood at 34 of Fahrenheit at noon.

XXXVI. *Remarks respecting Astronomical Observatories.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—Among the numberless advantages we derive from well-conducted periodical works on science and the arts, may be reckoned, as most important, not only the early communication of new and useful discoveries, exciting a praiseworthy spirit of philosophical inquiry, but likewise the facility with which we gain a pretty correct insight into the merits of different authors, their works and peculiar systems. Misrepresentations indeed may sometimes be permitted for a time; yet the opportunity and the inclination to do justice are seldom wanting, and the reign of error and falsehood is of short duration. A little opposition of opinion and amicable contention always contribute ultimately to the advancement of truth. Should an author, in the course of a long work, neglect to fulfill his early promises, or fail to continue his labours with the same assiduity and in as high a style of excellence as his readers had a right to expect from his commencement, where can such faults be pointed out to so much advantage as in a respectable periodical work on similar subjects? The author will be stimulated to fresh exertion, and future editions of his work will be improved; or, on the other hand, it will sink from competition with other works of a similar nature more deserving of public encouragement. I have here, as it were unconsciously, written a long and unnecessary preface, to introduce to your readers a few remarks on the last published part of the Edinburgh Encyclopædia, Part II. of volume xv. 1822. I first beg leave to quote the following observations from the history of the Circle, in volume vi. p. 485 of the same work. "The equatorial instrument constructed by Ramsden, for Sir George Shuckburgh, properly belongs to the class of astronomical circles. It will be more fully noticed under the article *Observatory.*—This brief introduction embraces all the principal astronomical circles which we know of in fixed observatories. It is, however, our intention to give, under the article *Observatory*, a more detailed account of some of them, particularly Mr. Groombridge's transit circle, and the mural circle now in use at Greenwich." Again, under the head "*Equatorial Instruments*," all that we find is "see *Observatory.*" Now the last part of volume xv. of this really valuable Encyclopædia contains the article *Observatory*, before referred to, and I anticipated that I should gain much useful information respecting those instruments, together with the best instructions for planning, building, and furnishing an observatory, considered either as a national establishment, or adapted to the means of a private individual. More than all, I hoped for an outline of the daily business of an observatory, calculated for the advancement of astronomy, pointing out the best methods of observing and recording celestial phenomena, and giving examples of the most accurate ways of reducing the observations, including of course the necessary tables, or else referring to them in some English publication. To this a catalogue of the astronomical observations already published would be no unacceptable addition. My disappointment may be imagined when I state that *all* these things are passed over in total silence; not a word of the promised equatorial; not a word of Mr. Groombridge's transit circle; not a word of the mural circle in use at Greenwich. Instead of the useful information laid up from the experience of former observers to guide and instruct future astronomers, the article in question really contains nothing but a meagre and incorrect history of observatories, almost wholly destitute of any practical value. It terminates with a table of the longitudes and latitudes of more than seventy observatories; and this part of the article I should approve, if it were as correct as it might have been. A good deal of valuable matter might have been collected from the works of the German, Italian and French astronomers, if the writer of the article had aimed at utility. To give an example of error, in page 446 it is stated that the business of the Vienna observatory is now conducted by Treisnecker, who succeeded Hell. But the truth is, that P. Treisnecker was succeeded, three years ago, by Mr. Littrow, and that the erection of a new Imperial observatory was to be commenced under his direction in the spring of 1821, agreeably to the plans of the Baron Zach, the veteran astronomer of Genoa. Littrow has found the latitude of his observatory = 48° 12′ 35″,45, instead of 48° 12′ 40″ as given in the table above mentioned *. Again, in page 447, the paragraph on the observatory of Malta states that the Chevalier d'Angos was a skilful astronomer, and made a great many valuable observations; while proof has been laid before the public, a considerable time since, by the indefatigable Zach, that d'Angos was a downright impostor, who invented observations, and with so little regard to theory and calculation as to be inconsistent with each other, and to deserve nothing but our hearty contempt for their author.

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* I think it will be found that the statement respecting the instruments in the Oxford observatory is in no inconsiderable degree redundant.

Hoping the few observations I have contented myself with making on the defects of the article *Observatory* in this valuable Encyclopædia will find a [illegible] in your Magazine, and produce a good effect, I remain, Gentlemen,

Your obedient servant,

Sept. 1822. A. M.

XXXVII. *A Letter to Professor* MILLINGTON, *of the Royal Institution, respecting some Frigorific Experiments made on the Magnetic Fluid, and on Sea-Water. By* B. DE SANCTIS, *M.D.**

SIR,—THE magnetic experiments of yesterday which I had spoken of to you the day before, have been successful beyond our most sanguine expectations. Mr. Cary (Strand), in whose house they were made, took a very active and intelligent part in them. But had you been present, as we expected, they would have been much more agreeable to us. Between two hollow parallelopipeds of laminated copper, four inches in length, four ditto in breadth, and one inch in depth, filled with ice and muriate of lime, and rendered air-tight with greased covers, the thermometer F being placed across them and its bulb resting on the cover of one of them, marked 40, the magnetic action of a common arrow-shaped needle, two inches long, 1-16 inch in the greatest breadth, weighing with the brass cap 7½ grs., was greatly paralysed in the open air, and also under the glass of the air-pump, and much more during and after exhaustion, when it stood still. The centre of the needle was at half the height of the parallelopipeds, and nearly at the distance of an inch and an half. Two other needles of the same shape, but three inches long, 1-8th in the greatest breadth, and weighing with the brass cap 11½ grs., showed still less activity, but not so much as the other. In trying a new needle, it was necessary to allow it sufficient time to adapt itself to the lower degree of temperature before it exhibited the *paralysis* of its forces. By this word I understand its being less sensible, or even altogether insensible, to the bar at the same distance and direction, and its sluggishness in returning to its former position, if it ever does so. The extremes of the scale of temperature, the thermometer being placed as before, were from 30 deg. at the moment of the most perfect vacuum to 40 deg. in the open air. Comparative observations on a more extended and detailed scale of temperature, particularly *in vacuo*, would be an interesting acquisition, and so much the more, as sufficient attention has not been paid to the temperature, when it has been said that the magnetic force augments *in vacuo* through the abstraction of the resistance of the air.

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* Communicated by Mr. Cary.

Now, sir, I am thoroughly convinced of the exactness of the indication, if not of the graduation, of the little watch-needle I had the honour of showing you at your lectures at the London Institution. That needle, amongst many other interesting facts, in the course of my last magnetic researches presented to me some time ago the same effects when plunged in a bath of ice and muriate of lime; but to avoid the objection that the movements of such a needle were not to be depended upon, and that they might be paralysed by the dampness of the air at so low a temperature, I wished to repeat the experiments at large, and in a vacuum, where certainly there was no dampness till the return of the air caused the vapours of liquefied ice which penetrated through the grease to fall upon the parallelopipeds, the needles and their supports—where a small degree of humidity was afterwards sensible, but only to the touch. This objection however at present can no longer exist, since I repeated at Mr. Cary's the same experiment on a larger needle similar to the lesser one above described. It was placed in a wooden case, likewise covered with glass, and protected from any effect of dampness by spreading the ground with dry powdered muriate of lime; and it was really interesting to see how soon the needle passed from activity to sluggishness at the alternate immersions and emersions from the frigorific mixture.

Amongst the many principles of reform in the construction of magnetic needles, of which I spoke to you, as being partly tried and partly to be tried, Mr. Schmalcalder agrees with me, that it would be perhaps better to destroy the action of the magnetic forces, in order to balance the inclination needle, by intense cold than by intense heat. The steel is always damaged by a high temperature, notwithstanding every precaution; and besides, I have seen practically enough in these matters to be able to assert, that even the incandescing heat cannot entirely destroy the magnetic power of any considerable mass of steel or iron, excepting loadstone of some particular mines; and even were it to happen momentarily in thin laminar steel, as soon as it cools the power begins to reappear. I was communicating the new project to Mr. Garden, of Oxford-street, when he very intelligently remarked that it would perhaps be better to try it *in vacuo*, as the humidity necessarily arising from a too low temperature in the air, might disturb the exact effect of gravity by its unequal precipitation on the needle and the augmentation of friction. In fact, that which was scarcely observable, even after the readmission of the air in the vacuum, at Mr. Cary's, I afterwards easily observed in the needles in the open air, by lowering more and more the temperature of the parallelopipeds, in consequence of many alternate and thinner layers of the mixture. At all events, therefore, I would recommend the trial *in vacuo*, with layers like the last, and with their ingredients reduced to the most convenient thinness.

But, waving the discussion till some practical observations give us more insight into the matter, let us rather relate how the magnetic forces decreased under the exhaustion of the air-pump, and how they increased on the readmission of the air. The decrease was slower than the increase; but the exhaustion too was slower than the readmission; and it was the same with respect to the progressive diminution and elevation of the temperature from 40 to nearly 30, and from 30 again to 40. But, notwithstanding the greater rapidity of augmentation, the air and the temperature once restored as before, the magnetic action seemed not restored to the same degree. I do not know whether this anomaly is to be deduced from an increase of friction depending on the precipitation of aqueous vapours, or from the protracted influence of the cold. How far could these two circumstances affect the oscillatory laws of the magnetic power when referred to the two opposite agents *in vacuo*, viz. the greater or less resistance of the medium combined with less or greater degree of temperature, and tried at different intervals through their different combinations? and what is the cause of such varying paralyses in needles? It is not an easy matter to answer the first of these queries, as depending very much on the delicacy of instruments and experiments; but as to the second, it seems the cause of the magnetic paralysis is not in the condensation of the air presenting an obstacle to the magnetic currents, as the effect equally takes place *in vacuo*: it seems the cause is not in the condensation of the steel, as it is commonly known that the greater the density of the steel, the more difficult, but the more great and tenacious, the intensity of the same currents once established, and besides, it could not so rapidly increase and decrease; therefore it is only, or at least principally, the action of cold on the magnetic fluid itself which produces the paralysis. Yes: as there are ices of rougher fluids, so there are more refined ices of the ethereal ones; and Des Cartes's mechanical principles will account as well for those which we may see and touch and taste, as for those which we can but barely discover by their relations with the movement of the needles.

Encouraged by the results of yesterday, I wished to try again this morning, in the open air, the effect of a low temperature on needles, the bars being out of the sphere of the cold; and, moreover, its effect on bars, the needles being in their turn out of the sphere of the cold; and lastly, the effect of the same on both of them at once. A more favourable stratification of the mixture allowed me to observe more decided effects at each combination, notwithstanding the general temperature of the room being lower than yesterday (both of them from 60°–70°), and notwithstanding I had no larger apparatus for cold than the former ones. They were, however, adequate to the graduated series of the bars, lying sometimes on their sides between the two parallelopipeds joined together, and sometimes separated only by their joined depths or lengths from the needles. I have even plunged needles and little bars into ice, and used both of them after drying them. The results have been always the same: the lower the temperature and the longer the immersion of the bars and needles, each separately or both at once, in the frigorific sphere, the greater the paralysis. What influence, therefore, must not be exercised on the currents of needles, as well as sometimes on the terrestrial ones, by the low temperature of the polar regions?—I am indebted for the idea of these experiments to the simple observation of Captain Ellis, who, meeting ice mountains in Hudson's Bay, saw his needles sluggish at their approach, and says that he restored them to their former activity by warmth. Far from reasoning on the circumstances, he has the air of repeating the fact as a kind of mysterious accident. What a difference between that transient rough observation and the results we have obtained! Such is the progress of science, aided by time, zeal for experiment, and skill in observing!

This morning, too, I have completed, with Mr. Garden, the experiments happily begun yesterday at Mr. Cary's about freezing sea-water with the same frigorific mixture used in the magnetic experiments. Sea-water from near South End (Essex), sold at the Establishment of the Sea-water Baths in George-street, Adelphi, freezes when in perfect quiet at only about 18° F., or at the utmost from 18° to 20°, when in small quantity and agitated on purpose. At 22° it was impossible to obtain ice through any length of time. What credit then shall we give to what Thomson says, even in the sixth edition of his "System of Chemistry," on "Nairne's authority from the Philosophical Transactions," that sea-water freezes at 28°·5 Fahrenheit? By the by, the general temperature of the room was rather high, to favour as much as possible the frigorific effects of the mixtures! The ice obtained from the water of the sea may be nearly deprived of salt, particularly when obtained without agitation, and well washed with cold water and dried on blotting paper; but animal and vegetable substances chemically or mechanically dissolved, impart a much deeper colour to the salt which remains after the complete evaporation of the washed liquefied ice, than of an equal quantity of the sea-water from which it originates. Therefore, if they are not more accumulating in the ice, they are at least the same quantity in a less proportion of salt; and consequently the colouring must be more strong. The colour of the dry salts certainly did not depend upon iron, as their solution in distilled water was not sensible in the least, either to the gallic acid or to the prussiate of potash. It follows naturally from these trials, that fresh water for kitchen uses and washing may be obtained from sea-water better by distillation than by freezing; and particularly if care be taken to purify the distilled water further by filtering it through sand and powdered charcoal, and to impregnate it afterwards with a small quantity of carbonic acid gas, which would take off its flatness, and make it more palatable. By these precautions perhaps all bad effects would be better avoided, should they even be as great as those described by some physicians of the South, who had an opportunity of seeing the effects of the long-protracted use of that water on some criminals, and which have never been observed in England upon any occasion of its use, not even when, at a large dinner of two hundred persons, South-End water was the only water used even for ices. But sea-water taken at the same depth in the southern seas might perhaps be more charged with noxious substances than the sea-water of South-End: and, besides, there is a wide difference between the use of it for one day at a great dinner, and for weeks together with frugal fare. But, be that as it may, it will always be of some interest to know that with a small quantity of ice, and muriate of lime, a great deal of ice might be produced in a vessel without any waste of fresh water or ice fit for use.

Some couples of thin lead cylinders, or perhaps better parallelopipeds, placed the one within the other, would suffice. Fill the internal one with sea-water; fill with alternate layers of pounded ice and muriate of lime, which needs not absolutely to be powdered, the little space between the two; and keep them quiet. After some time, always depending on the more or less advantageous disposition of the dimensions of the recipients, on the general temperature of the air, and the former temperature of the water (which ought to be taken as little warm and salt as is allowed by the means at hand and the depth of the sea) light clouds shall be seen floating all along the water. This is the moment of helping the crystallization by lightly moving the water with a stick. Thus lumps of ice are obtained sufficiently large and thick to be taken out of the recipient and fit for resisting fusion. The ice, once well fixed, may serve partly for immediate use, and partly to reproduce ice without breaking in upon the ship's store. The first liquefied ice may be useful for cooling, were sea-water to be frozen in another recipient, if it is no longer fit for the same purpose, and so on.

The muriate of lime is easily collected for further use from the sweet liquefied ice by evaporation; but it is not so with the muriate of lime dissolved by sea-water ice. It mixes then with the salt of the sea-water, and after evaporation they remain united. But there is muriate of lime too in the sea-water, and muriate of magnesia; and if the muriate of soda is not so favourable to the frigorific mixtures as the others, it will always be of some use when mixed with a sufficient quantity of the muriates of lime and magnesia, as used by the confectioners, and certainly will never be unfavourable to cooling. The sweet ice ought to be kept in wooden cylinders within leaden ones, with a sufficiently deep stratum of powdered charcoal between them to preserve a constant temperature. The muriate of lime ought to be put in earthen jars well corked up. The driest and coldest place of the ship is the best for the ice; the driest and warmest for the muriate of lime. I shall not recommend to seamen to collect the salt employed in a state of crystallization; they would scarcely understand the name, and not at all the management, and not much more than the name and the management of the air-pump for obtaining ice: but I would recommend it to any chemical man on board hospital-ships, for the use of which the proceeding is intended more than for increasing the luxuries of vessels, as the effect of the first part of water taken even by the deliquescent salts would tend rather to increase the temperature. But the passage is so rapid to the contrary effect, and such is the degree of its power, that the circumstance is not worthy of notice with seamen. I said "sea-water ice might be obtained nearly deprived of salt," because it is impossible to separate it entirely from it. The cold water would carry away the very salt water adhering to its surface; but it cannot carry away the similar drops inclosed inside without destroying the ice itself. These drops are found here and there in the thickest pieces of ice; and let the ice drain as it will, let it be washed and washed again with cold water, and dried and dried again on blotting paper, the greatest part of the internal drops will always remain inside, at least in our laboratories, and particularly if the ice has been obtained with agitation.

Might not this observation shed some light on the origin of the great masses of sweet ice of the polar seas? I would not say that that ice is not of maritime origin on account of its sweetness, as there is reason to believe that sweet ice might be formed through seas where the salt is diminishing as they approach the poles, particularly where at a stationary low temperature the less salt or nearly sweet water might freeze without agitation; and, besides, water descending from the air might contribute to its increase. But, besides fields floating from the North, I should incline to refer to the frozen waters of rivers and lakes, dissolved by thaw, a great quantity of those pieces of ice which encumber the shores of less sweet seas. The ice of their water cannot be deprived entirely of its salt, and particularly when formed with agitation, as would be the case at any degree above 18°. Meanwhile those ices have been found every where sweet by the majority of voyagers, and so we receive them, from the shores of Norway and Iceland. I would finish here, but allow me to add that the specific gravity of the sea-water of our experiments is 1·020.

I am, with respect, sir,
Your most obedient servant,
B. De Sanctis, M.D.

2, Foubert's Place, Regent-street,
June 21, 1822.

To Professor Millington.

P.S.—After having written this letter, two other reflections occurred to my mind, which seem not unworthy of being mentioned in addition to the former ones.

1. Should the process of the sea-water distillation ever be rendered, by particular construction, more easy and not at all troublesome to seamen, the distilled water might perhaps undergo a further improvement under *iceing;* it is an experiment at least to be tried at large. The vegetable and animal substances which seem not to be disposed to separate from the crystals of the sea-water, might perhaps separate where they are not surrounded by any salt drops. Besides, when the sweet ice of the ship was nearly finished, a new provision might perhaps be obtained from the iceing of the distilled water more fit for resisting fusion than the salt ice. At any rate, the muriate of lime could always be obtained again clean and equally powerfulfrom its solution in the distilled sea-water: which circumstance is rather of importance.

2. Might not the electrical discharges through the cold air of the polar regions favour the crystallization of the aqueous vapours of the atmosphere, as they favour the formation of the frost and snow and hail in our apparatuses? Such discharges must be very frequent in the polar regions, as the very rotation of the earth and of the surrounding air must necessarily develop electricity

in the higher regions of the atmosphere, where it is equally spread, and where it must naturally be compelled towards the poles by the perpetual current of the air from the equator towards them. Such an accumulation not only might furnish the reason of the frequency of that decidedly electrical phænomenon which is called *aurora borealis* on the North, and which is not less rare towards the Southern Pole, but might perhaps render a just reason for the great part which the atmospherical water might take in the formation of the inexhaustible ices of the polar regions.

XXXVIII. *On Lithographic Printing.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—HAVING found considerable inconvenience arise from the use of grease on the surface of the stretched leather in the tympan frame, over which the scraper passes, —on account of the dirt it creates, the injury it occasions to the leather, and the waste of paper,—I have tried various substitutes.

The most successful experiment I have yet made, has been with Castile soap rubbed over the leather with a little water. It very speedily produces, by the action of the scraper passing over it, a glossy surface; and I feel confident that the labour in working the press is even less than when grease is employed. This, in addition to the other advantages it possesses, viz. cleanliness and œconomy, strongly recommend its use to those employed in lithographic printing—particularly those who practise it as an amusement.

I am, gentlemen, your obedient servant,
CHARLES M. WILLICH.

The Lithographic Press,
8, Pickett-street, Strand, Sept. 23, 1822.

XXXIX. *Short Account of the Rocks in the Neighbourhood of St. John's, Newfoundland. By Mr.* JOHN BAIRD[*].

IN approaching the fishing grounds on the coast of America, the soundings were from sixty to thirty fathoms; over the great Bank of Newfoundland, generally about thirty-five. The lead brought up a fine sand, and frequently small pieces of a rough flint, together with particles of a green smooth mineral, in the form of coarse green sand. It is certainly a singular fact, that so large a portion of shallow water should exist so far out at sea, the sea deepening so rapidly beyond the Bank. Is it not probable that a large tract of dry land had formerly existed where the Bank is now found? The rocks which formed this land may have been composed of very soft materials, and the occurrence of flint and green sand over the bank seems to indicate that the greater part of the original rocks had been of the chalk and green sand formations. A country composed of these rocks, which are of the latest formation, must have been very low, and in consequence much exposed to the action of the sea. By degrees the whole may have been inundated and entirely swept away, leaving the harder debris, the flint and green sand, to form the gravel at the bottom.

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[*] From the Memoirs of the Wernerian Natural History Society for 1821-22. Vol. iv. Part I.

The coast round St. John's is bleak, bare, and rocky, and almost every where precipitous. On both sides of St. John's harbour, perpendicular cliffs of trap-tuff rise to the height of three or four hundred feet. The interior of the country is hilly, but does not rise to a great elevation, few of the hills being more than five or six hundred feet above the level of the sea. They are generally round-backed, and frequently wooded to the top. The whole uninhabited part of the country is one immense forest, consisting chiefly of fir and birch. No extensive valleys or plains occur, but hill succeeds hill in almost unvarying succession. The physiognomy of the country is an elegant outline of hill and dale: the scenery, however, wants variety. Lakes are numerous over the whole island, as far as it is known, and many of them, near the coast, are large and beautiful. They occur even on the tops of the hills, and are said to be often of great depth. The soil is in general light. Oats and barley thrive; and potatoes, turnips, and other kitchen vegetables, grow fully as well as in England. Summer weather, in Newfoundland, is short, but warm, and very favourable to vegetation. The winter is uncommonly severe, the spring and autumn very changeable. There is good pasture for cattle in ground that is cleared.

The passage into St. John's harbour, which in shape very much resembles a man's foot, is by a narrow entrance, called The Narrows, which extends nearly east and west about half a mile. Both sides of this entrance are high, abrupt, and rocky. The rocks are the same on each side, being continued across from the one to the other. I think there is little doubt that the opposite sides of the Narrows had once been joined. A rapid river runs into the harbour. The harbour itself, previously to the formation of the Narrows, may have been a lake. The river appears to have been once much larger. By the action of the sea without, and of the river and lake within, the rent or chasm by which the river formerly emptied itself into the sea, may have been gradually enlarged, till it has attained its present size. The average width may be two or three hundred yards. Quidi Vidi (pronounced *Kitty-vitty*) river and lake may one day present an entrance and harbour similar to those of St. John's.

The mineralogy of the country round St. John's is very simple. The first rock, on entering the Narrows, is trap-tuff. This rock is distinctly stratified, each stratum generally measuring two or three feet in thickness. The strata lie NE. and SW., or rather NE. by N. and SW. by S. The dip is to the NW., at an angle of from 70° to 80°. The basis of this rock consists chiefly of distinct grains of quartz, felspar, and a red claystone. The imbedded minerals or pebbles are, for the most part, of the same substances; felspar, common and compact, the latter with small imbedded grains of quartz; quartz, often of a slaty or fibrous structure; jasper, red claystone, bloodstone, hornstone, &c. These imbedded minerals, at the foot of the hill or cliff (particularly the quartz and felspar), are generally from an inch to three inches in diameter, and gradually decrease as we ascend; at the top of the hill, they rarely exceed the fourth part of an inch in diameter. Does not this fact countenance the mechanical deposition of the trap-tuff? This rock appears much harder than the common varieties of the trap-tuff which I have seen.

It is this rock which forms those precipitous cliffs on either side of *the mouth* of the Narrows. The opposite sides of the Narrows rise very rapidly from the sea to a considerable height. The highest part of the trap-tuff formation is about 300 feet above the sea; its thickness about 500 yards. The trap-tuff passes very gradually, and most beautifully, into the next rock, which is amygdaloid. In this passage of the one rock into the other, the stratified structure is still retained, one stripe or narrow stratum being distinctly marked trap-tuff, the next amygdaloid; the stripes of the former being broadest at first, gradually becoming narrower and less defined, till the amygdaloidal rock entirely prevails.

The strata of the amygdaloid also run in the direction of NE. by N. and SW. by S., and dip likewise to the NW. at an angle of about 65°. The basis of this rock, like that of the trap-tuff, consists of minute grains of quartz, felspar, and claystone. The imbedded portions are invariably of a red, smooth, hard claystone: they seldom have the amygdaloidal form, but are square, or rhomboidal, or in longish slates. The greater part of the amygdaloidal rock is entirely destitute of these portions, while, on the other hand, more than one-half of some of the strata is composed of them. The amygdaloid is frequently distinctly stratified, each stratum being a few feet thick.

The trap-tuff and amygdaloid are both of a reddish colour; the latter, however, sometimes occurs of a grey colour. The greatest height of the amygdaloid formation is 500 feet*; its thickness is about three or four hundred yards, extending from the top of Signal Hill to the foot of the Crow's Nest.

Resting upon the amygdaloid is found the greenstone, lower in height than the amygdaloid, but higher than the trap-tuff. This rock extends from the foot of the Crow's Nest (it being of greenstone) to the foot of the Signal Hill, or to the town of St. John's, a thickness of six or seven hundred yards. The Crow's Nest, on which is built a small fort, is four hundred feet above the level of the sea. The principal constituent part of this rock is apparently felspar. Its most common colour is green, though sometimes gray and red; it is stratified, and sometimes possesses a beautiful slaty structure. The strata of the greenstone also run NE. and SW.; their dip is to the NW., at a much smaller angle than the preceding rock, the inclination not exceeding 50°.

Resting upon the greenstone we find the next rock claystone, the strata of which have the same direction and dip as the others, the angle of inclination, however, not being above 35° or 40°. The claystone formation extends two miles in thickness beyond the greenstone, occasionally alternating with strata of compact felspar, each stratum measuring from half a foot to a foot in thickness. The claystone being much softer than the rocks before described, the country where it prevails is also much lower. The town of St. John's is built upon the claystone. In colour, it is most frequently gray, often also brown, dark-brown, red, whitish, and of other colours. It is often beautifully striped. It is fine-grained, smooth, and often conchoidal in the fracture. The strata of this rock are occasionally columnar, which is also sometimes the case with the greenstone; and the columns are composed of round concentric balls.

The next rock, whose direction, dip, and inclination are the same as those of the claystone, is compact felspar. This rock first alternates with the claystone, and then prevails alone for above a mile. Being harder than the claystone, the country composed of it is higher. Its colour is also various, light and dark gray, greenish-gray, green, blue, &c. It is translucent, or slightly translucent, on the edges, while the claystone is perfectly opaque. The compact felspar has a splintery conchoidal fracture; the fracture of the claystone, on the other hand, is even, and always smooth. The compact felspar is more or less fine in the grain, and the splinters more or less large. The strata of the compact felspar, like those of the greenstone and claystone, are also at times columnar, the columns being composed of small round concentric balls, and very brittle. These balls are at times partially composed of hornstone. This mineral occurs also in the compact felspar, in thin beds, in veins, and in masses; its colour is green, its fracture smooth, conchoidal, it is slightly translucent on the edges, and is as hard as quartz.

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* This is the height of Signal Hill, the highest part of the formation.

Claystone again succeeds the compact felspar, and the latter is again succeeded by the former, thus alternating for about eight or ten miles across the peninsula. The claystone always possesses a slaty structure, and soon decays. The soil over the greenstone and amygdaloid is rich and good, while over the claystone and compact felspar it is light and poor.

XL. *Reply to Captain* FORMAN'S *Theory of the Tides.* *By Mr.* HENRY RUSSELL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—THE expansibility of water being a well attested fact, Captain Forman's theory will no doubt meet with due attention; but I am persuaded that very few will be delighted with his method of reasoning.

With regard to his question, "Do the waters at the time of their rising press downwards, or do they not?" I answer, They press downwards; but with as much less power as is equal to the attractive power of the moon.

To the next question, "How are we to account for their rising, except by supposing that they are pressed upwards by the expansion of the particles below?" I answer, By the superior gravity of those waters which constitute the ebb.

I do not deny an expansion and contraction of the waters occasioned by the arrival and departure of the moon; but the circumstance of the highest tides being invariably accompanied by the lowest ebbs, is alone sufficient to convince any impartial inquirer, that the ebb and flow of the waters are produced by changes of place, and not by rarefaction and condensation alone.

The satisfaction which Mr. Forman seems to derive from a handful of water is by no means enviable. He speaks of the moon as if astronomers and philosophers think its attractive power over substances upon the face of the earth equal to the attractive power of the earth itself; but I believe there are very few philosophers who are not satisfied of the contrary. Surely he does not wish us to understand, that because the moon has not power to sustain a handful of water in the atmosphere, it has no power over it whatever. He may as well attempt to teach us, that because a magnet has not power to lift a scale-beam, it has not power to disturb its equilibrium. Upon re-examining the subject, Capt. Forman may possibly discover, that the altitude of the flow is to the altitude of the ebb, as the gravity of the ebb is to the gravity of the flow.

HENRY RUSSELL.

XLI. *Notices respecting New Books.*

The Fossils of the South Downs; or, Illustrations of the Geology of Sussex. By Gideon Mantell, F.L.S. The Engravings executed by Mrs. Mantell, from Drawings by the Author. 4to, pp. 327. London 1822.

THE infant science of Geology is making rapid advances, and must continue to do so when embraced with such ardour as is displayed in the volume before us. Mr. Mantell, to whom the public is indebted for describing, and that very minutely, the geological phænomena of an unexplored part of Sussex, is a surgeon residing in the county, who, amid the numerous and anxious duties of his profession, has snatched a few moments which he has consecrated to science; but he has done more, he has enlisted his amiable partner in the same cause; and forty-two well engraved plates attest Mrs. Mantell's talents as an engraver, as well as her love of science, to which she has thus so liberally contributed.

Mr. Mantell's work is preceded by a preliminary Essay, written by a Clergyman of the Established Church, to prove the correspondence between the Mosaic account of the creation and the geological structure of the earth.

Although Mr. Mantell at first intended to confine his researches to the South Eastern division of Sussex, yet he has extended them to nearly the whole county, which he describes very carefully and very minutely in all its various formations. He confines himself almost entirely to facts, seldom offering an opinion on a disputed point, but rather wishing to show the sentiments of others.

We shall not enter into the details, which are so well illustrated by the plates accompanying them, but quote from his concluding observations, the inference which he draws from the whole:

"1stly. That the strata composing the county of Sussex have been formed, at different periods, by successive depositions at the bottom of tranquil seas*.

"2dly. That the waters which deposited these formations were inhabited by shell-fish, zoophytes, fishes, &c., the greater part of which were not only essentially distinct from any that are known in a recent state, but many of them are confined to certain deposits.

"3dly. That one of these formations (the Tilgate beds) contains the remains of shells, fishes, palms, arborescent ferns, turtles, gigantic lizards, and unknown quadrupeds; an assemblage of organic remains for which it is difficult to account, unless we suppose that the bed in which they are inclosed was deposited by a river or lake of fresh water.

"4thly. That the chalk subsequently to its consolidation has suffered extensive destruction: the upper beds having been swept away, and extensive basins formed on its surface.

"5thly. That the excavations, or basins of the chalk, have been filled up by a series of depositions, possessing very different characters to any that preceded them; and which in some places (Isle of Wight, Paris, &c.) consist of alternations of marine and fresh water deposits.

"6thly. That these newer depositions have also been broken up, and in a great measure destroyed, by an irruption of water in a state of violent commotion; a catastrophe to whose powerful agency the present form of the surface of the earth, and the accumulations of beds of gravel, sand, &c. are to be attributed.

"7thly. That it is only among these last and newest deposits, the wrecks of ancient formations, that the remains of the elephant, deer, horse, and other land quadrupeds, have hitherto been discovered.

"Lastly. That the present effects of the ocean appear to be wholly inadequate to produce changes like those which have formerly taken place.

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* The absence of all traces of land animals and vegetables in these beds, does not however appear to warrant the inference, that the former were not then in existence. For if we suppose that, after the deposition of the iron sand, the sea retired, and the surface of that formation became clothed with vegetation, and inhabited by animals; may it not be presumed, that if the approach of the next ocean was gradual, the advance and retrocession of its waves might destroy all traces of the land and its productions, before the water covered the surface to a sufficient depth to allow of the tranquil deposition of the Weald clay? This remark equally applies to the other secondary formations.

" Hence it appears, that in the lapse of ages, the sea alternately encroaches on, and retreats from the land, and the districts it formerly occupied become the habitation of terrestrial animals and vegetables ;—but other revolutions succeed, the sea returns to its ancient bed, and the countries from which it retires are again fitted for the reception of their former inhabitants."

Mr. Mantell has previously given an account of the present effects of the ocean on the Sussex coast. On this subject, he says, " The present operation of the sea seems to be wholly incapable of producing the important changes that have formerly taken place, and on the Sussex coast they are restricted to a gradual but constant destruction of the strata which compose its shores.

" The encroachments of the sea along the coast of Sussex, have continued incessantly from time immemorial; and when so considerable as to have occasioned sudden inundations, or overwhelmed fertile or inhabited tracts, have been noticed in our historical records. In the *Taxatio Ecclesiastica Angliæ et Walliæ, auctoritate P. Nicholas,* (A.D. 1292,) and *Nonarum inquisitiones in curia scaccarii,* (A.D. 1340,) the following notices occur of the losses sustained by the action of the sea between the years 1260 and 1340; a period of only eighty years *.

" At Pett, marsh land overflowed by the sea; the tithes of which were valued at two marks per annum.

" Iklesham and Ryngermersh, lands of which the tithes were 49*s.* 8*d.* per annum.

" Thornye, 20 acres of arable, and 20 acres of pasturage.

" Selseye, much arable land.

" Felpham, 60 acres of land.

" Middleton, 60 acres.

" Brighthelmston, 40 acres.

" Aldington, 40 acres.

" Portslade, 60 acres.

" Lancing, land the tithes of which were 44*s.* 6*d.* per ann.

" Siddlesham and Westwythering, much land.

" Houve, 150 acres.

" Terringe, land, the tithe valued at 6*s.* 8*d.* per annum.

" Bernham, 40 acres.

" Heas, 400 acres.

" Brede, great part of the marsh called Gabberghes.

" Salehurst and Udimer, land the tithes of which were valued at 40*s.* per annum.

" At Brighton, the inroads of the sea have been very extensive. In the year 1665, twenty-two tenements *under* the cliff had been destroyed, among which were twelve shops, and three cottages, with land adjoining them. At that period there still remained *under the cliff* 113 tenements; and the whole of these were overwhelmed in 1703 and 1705. Since that time, an ancient fort called the Block-House, with the Gun-garden, wall, and gates, have been completely swept away, not the slightest trace of their ruins having been perceptible for the last 50 years*.

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* I was favoured with this notice by the late Thomas Woollgar, Esq. of Lewes.

"At the present time, the whole line of coast, between the embouchure of the Arun, and Emsworth harbour, is visibly retreating, and the means adopted for its prevention have hitherto been attended with but little success†.

"The process by which this destruction of the coast is effected, is sufficiently obvious. By the incessant action of the waves the cliffs are undermined, and at length fall down, and cover the shore with their ruins. The softer parts of the strata, as chalk, marl, clay, &c., are rapidly disintegrated and washed away; while the flints, and more solid materials, are broken and rounded by the continual agitation of the water, and form those accumulations of sand and pebbles that constitute the beach, and which serve, in some situations, to protect the land from further encroachments. But when the cliffs are entirely composed of soft substances, their destruction is very rapid, unless artificial means are employed for their protection; and even these, in many instances, are but too frequently ineffectual."

Petworth in Sussex is celebrated for its marble; the quarries are nearly four miles from the town, and the marble is found in a horizontal bed of blue clay at the depth of twenty-five feet. It is divided by fissures into large slabs, fit for tables and other purposes, varying in thickness from twelve to twenty-two inches. These beds of marble traverse the country in a N.N.W. direction, extending from Kirdford in Western Sussex to Laughton, six miles N.E. from Lewes, whence it proceeds eastwardly, and is lost in the alluvial marshes of Pevensey levels. Of this marble Mr. Mantell gives the following account:

"This limestone is of various shades of blueish grey, mottled with green, and ochraceous yellow, and is composed of the remains of fresh water univalves, formed by a calcareous cement into a beautiful compact marble. It bears a high polish, and is elegantly marked by the sections of the shells which it contains. The shells belong to the genus *Vivipara* of Montfort,

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* Lee's Hist. of Lewes and Brighton.

† Dallaway's Western Sussex, vol. i. p. 55.

(*Helix vivipara*, of Linné,) and are supposed to resemble the recent species of our rivers: their constituent substance is a white crystallized carbonate of lime, and their cavities are commonly filled with the same substance, presenting a striking contrast to the dark ground of the marble. In other varieties the substance of the shells is black, and their sections appear on the surface in the form of numerous lines and spiral figures.

"The Sussex marble occurs in layers, from a few inches to a foot in thickness, and these are commonly separated from each other by thin seams of clay, or of coarse friable limestone. It is frequently found in blocks or slabs, sufficiently large for sideboards, columns, or chimney-pieces, and but few of the ancient residences of the Sussex gentry are without them. There is historical proof of its having been known to the Romans, 'and in the early Norman centuries it was much sought after, and applied as the Purbeck marble was, when cut into small insulated shafts of pillars, which were placed in the *triforia* or upper arcades of cathedral churches, as at Canterbury and Chichester. At the first-mentioned, the archiepiscopal chair is composed of it. Another more general use was for slabs of sepulchral monuments, into which portraits and inscriptions of brass were inserted. In the chancel at Trotton, there is a single stone, the superficial measure of which is nine feet six inches by four feet six inches; and another, in the pavement of the cathedral of Chichester, measures more than seven feet by three and a half*.' York Cathedral, Westminster Abbey, Temple Church, Salisbury Cathedral, and most of the principal gothic edifices in the kingdom, contain pillars or slabs of this marble. It is singular, that in Woodward's time, an opinion prevailed, that these pillars, &c. were artificial, and formed of a cement cast in moulds; but, as that author remarks, 'any one who shall confer the grain of the marble of those pillars, the spar, and the shells in it, with those of this marble got in Sussex, will soon discern how little ground there is for that opinion, and yet it has prevailed very generally. I met with several instances of it as I travelled through England, and had frequent opportunities of showing those who asserted these pillars to be factitious, stone of the very same sort with that they were composed of, in the neighbouring quarries†.'"

The Plates to this work, several of which are neatly coloured to exhibit the various strata, are all well engraved; and with the letter-press form a valuable illustration of the Geology of Sussex.

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* Dallaway, ch. xxvi. p. 115. † Woodward's Fossils.

UPSAL:—*The Eighth Volume of the Memoirs of the Royal Society* has just appeared, under the title of "Nova Acta Regiæ Societatis Scientiarum Upsaliensis."

The following are the papers on scientific subjects:—*Petrificata telluris Succanæ*, by Dr. Wahlenberg.—*De reductione quantitatum imaginariarum*, by the Chev. de Nordmark.—*Coleoptera capensia antennis fusiformibus*, by M. Thunberg.—*Ovis polyceratæ varietates*, by the same.—*Alurni tres novæ species*, by the same.—*Gothlandiæ plantæ rariores*, by MM. Rosen and Wahlenberg.—*Monographia chlythræ*, by Dr. Forsberg.—*De Gyrinis commentatio*, by the same.

The First Part of Volume I. *of a new Series of the Transactions of the Geological Society* has just appeared. It contains the following articles, with 24 Plates.

I. An Outline of the Geology of Russia. By the Hon. Wm. T. H. F. Strangways, M.G.S.—II. Remarks on the Geology of the South Coast of England, from Bridport Harbour, Dorset, to Babbacombe Bay, Devon. By H. T. De la Beche, Esq. F.R.S. and M.G.S.—III. On the Bagshot Sand. By Henry Warburton, Esq. F.R.S. V.P.G.S.—IV. Supplementary Remarks on Quartz Rock. By John Mc Culloch, M.D. F.R.S. F.L.S.—V. Additional Remarks on Glen Tilt. By John Mc Culloch, M.D.—VI. On the Geology of the Coast of France, and of the inland Country adjoining; from Fecamp, Department de la Seine Inferieure, to St. Vaast, Department de la Manche. By H. T. De la Beche, Esq. F.R.S. and M.G.S.—VII. On a Fresh-water Formation in Hordwell Cliff, Hampshire; and on the subjacent Beds, from Hordwell to Mudiford. By T. Webster, Secretary to the Geological Society.—VIII. On the Excavation of Valleys by diluvian Action, as illustrated by a Succession of Valleys which intersect the South Coast of Dorset and Devon. By the Rev. Professor Buckland, F.R.S. V.P.G.S.—IX. Additional Notices on the Fossil Genera Ichthyosaurus and Plesiosaurus. By the Rev. William D. Conybeare, F.R.S. M.G.S. &c.—X. On the Valley of the Sutluj River in the Himálaya Mountains. By H. T. Colebrooke, Esq. F.R.S. V.P.G.S.—XI. On the Geology of the North-eastern Border of Bengal. By the same.—Appendix. On the Banks of the Tistá and Súbúk Rivers. From a Notice by D. Scott, Esq.—XII. Description accompanying a Collection of Specimens made on a Journey from Delhi to Bombay. By James B. Fraser, Esq.—XIII. Notices and Extracts from the Minute-Book of the Geological Society respecting Fossil Plants found at the Col de Balme, near Chamouny in Savoy. By H. T. De la Beche, Esq. F.R.S. &c.;—Accompanying Specimens of Lead Ore, found in Toadstone, from near Matlock, Derbyshire. By Charles Stokes, Esq. F.R.S. &c.;—Respecting a Copper Mine at Cally in Kircudbrightshire. By John Taylor, Esq. M.G.S.;—Respecting the Rocks of the Islands of Penang and Singapore, Straits of Malacca. Part of a Letter from Mr. Wm. Jack, to H. T. Colebrooke, Esq. V.P.G.S.;—A recent Deposit of Compact Limestone. By Charles Stokes. Esq. F.R.A. and L.S. M.G.S. &c.;—Accompanying Specimens from the Neighbourhood of the Gáró Village of Robagiri. From a Letter from D. Scott, Esq. to H. T. Colebrooke, Esq.;—On Fuller's-earth found in Chalk in Sussex. By F. Sargent, Esq.;—On the Black Oxide of Manganese of Warwickshire. By Samuel Parkes, Esq. F.L.S. M.G.S. &c.;—On the Gypsum of Monte Seano. By M. Scipio Breislak, &c. &c.;—On the Rocks of Attica. By Joseph Woods, Esq. F.L.S. M.G.S. &c.;—Notice accompanying Specimens from the Bermuda Islands. By Capt. Vetch, M.G.S.;—Notice on some Fossil Shells, from Langton-Green, near Tunbridge-Wells. By Sir Alexander Crichton, F.R.S. M.G.S. &c.

Part I. *Volume* IV. *of The Memoirs of the Wernerian Natural History Society of Edinburgh* for 1821–22, is just published, and contains the following papers:

By W. Haidinger, Esq. On the Crystallizations of Copper-Pyrites.—Notice of the Attempts to reach the Sea by Mackenzie's River, since the Expedition of Sir Alexander Mackenzie.—By Dr. Adam. Geological Notices, and Miscellaneous Remarks, relative to the District between the Jumna and Nerbuddah; with an Appendix, containing an Account of the Rocks found in the Baitool Valley in Berar, and on the Hills of the Gundwana Range; together with Remarks made on a March from Hussingabad to Sangar, and from thence to the Ganges.—By Robert Bald, Esq. Notices regarding the Fossil Elephant of Scotland.—By Robert Kaye Greville, Esq. Descriptions of Seven New Scottish Fungi:—A Description of a new Species of Grimmia, found in Scotland:—and a Description of two new Plants of the Order *Algæ.*—By the Rev. W. Macritchie. Meteorological Journal, kept at Clunie, Perthshire, for 12 Years, from 1809 to 1820.—By Dr. Amie Boué. On the Geognosy of Germany, with Observations on the Igneous Origin of Trap.—By R. K. Greville, and G. A. Walker Arnott, Esqs. A New Arrangement of the Genera of Mosses, with Characters, and Observations on their Distribution, History, and Structure.—By Mr. John Baird. Short Account of the Rocks in the Neighbourhood of St. John's, Newfoundland.—

By L. Edmondston, Esq. Observations on the Snowy Owl, (*Strix Nyctea*, Linn.):—Account of a New Species of Larus, shot in Zetland:—Additional Account of the Iceland Gull:—and Observations on the Immer Goose of Zetland.—By Capt. R. Wauchope, R.N. Meteorological and Hydrographical Notes.—By Mr. G. Anderson. Account of the Small District of Primitive Rocks, near Stromness, in the Orkney Islands;—Geognostical Sketch of Part of the Great Glen of Scotland.—By Mr. W. Macgillivray. Notice relative to two Varieties of *Nuphar lutea*, found in a Lake in Aberdeenshire.—By the Rev. J. Grierson. Some Observations on the Natural History of the Mole.—By Captain Vetch. Account of the Island of Foula.

A Treatise on the Foot-rot in Sheep; including Remarks on the exciting Cause, Method of Cure, and Means of Preventing that destructive Malady:—being the Substance of Three Lectures, delivered in the Theatre of the Royal Dublin Society. By Thomas Peall, Esq. Veterinary Professor to the Society. Dublin: printed by Joshua Porter, Grafton-street.

This is not only a scientific but (what all scientific books are not) a highly *useful* and practical work. Mr. Peall opens with a minute investigation of the structure of the sheep's foot. If in the course of his remarks Mr. P. should express himself in strong terms on "the mass of absurdity and error," the "preposterous reasoning," &c. which have been poured forth on this subject, we can scarcely wonder; knowing, as we do, the great ignorance that prevails as to the commencement of the disease, and being well aware, with Mr. P., that it is seldom attempted to be cured until it has attained even to suppuration. But of all the errors which have been promulgated on the question of foot-rot, from the *Encyclopædia Britannica* downwards, none strikes us as so absurd, so extraordinary, as those which obtained respecting that "very remarkable gland, or rather glandular sac, which is found situated at the lower surface of, and between, the bones which articulate with the foot bones." This little opening or duct (to be seen between the claws of every sheep) has been by divers authors supposed to be *the disease*, occasioned by a certain worm, which has not inserted itself into the foot *by means* of, but, as they infer, actually formed, this opening; and particular instructions are given in divers works how to extract the worm. From Mr. Peall, the reader will learn (and it is not the least important part of his book) the use and importance of this organ; which, so far from being what it has been ignorantly supposed to be, is, says he, "one of the most singular apparatuses that I have ever met with for preventing friction between the parts where it is situated."

The supposed worm is simply "hardened mucus, which frequently plugs up the pipe," neither more nor less;—the effect, observe, and not the cause of disease. Having dwelt at some length on this part of the subject, Mr. P. proceeds to give us his "theory" of the complaint; and we think he has demonstrated (to our satisfaction at least) that the grand exciting cause is *cold*, or, rather, cold and wet combined. "From the increased action of the blood-vessels, a slight degree of inflammation, and one of its concomitants, heat, results. This may be considered the first stage of the disease. Then it is that the sheep begins to limp a little: but as it seldom happens that the animal is attended to, or removed to a drier spot, whilst the lameness is so slight, the inflammation commonly passes on to the second or suppurative state; and then the horn may be observed to begin to separate from the quick, attended with an oozing of matter at the upper edge of the claw, exactly at the spot where the horn is lost (as it were) in the skin."

"Phenomena of a similar kind are observed to take place in chilblains, which, it will be admitted, arise from the effects of cold." No one however, in his senses, would wait till the chilblains have broken, before attempting a cure: neither should he (if this analogy be correct) in the case of foot-rot. The idea so common, of the infectious nature of this disease, Mr. P. has completely and satisfactorily refuted. From the horny construction of the foot, the thing is impossible.—We regret that our limits will not enable us to treat this part of the work with the attention it so well deserves: but it is a cheap publication, consisting of but 55 pages, and consequently within the reach of every sheep-breeder, bad as the times are. Before we take leave of Mr. P., however, we think it but fair, having given the outline of his opinion as to the nature of the disease, to state as briefly as possible his "remedy therefore."

"As soon as a sheep is observed to limp, the feet ought to be immediately inspected; and if no mechanical cause of lameness can be discovered, then the ordinary seat of foot-rot,—namely, the inner portion of the claw, near the coronet,—should be examined; and if any oozing of matter at the upper edge of the horn appear, the detached portion should be carefully removed from the quick with a *sharp crooked* knife." Now in what this *crookedness* consists, we know not: a knife may be very *crooked*, and yet far from being *properly* crookt for such a purpose,—*sharpness* we know to be indispensable, or injury may be done by jagging the parts affected. The indiscriminate use of liquid caustics is very properly reprobated: "yet it has been found that the muriate (usually called the butter) of antimony is an excellent remedy in foot-rot, especially if it be diluted with an equal quantity of saliva, or of white of egg, and then applied by means of a small brush to the ulcerated surface, after all the loose detached horn has been *scrupulously* cut away." Mr. P. also deprecates the practice of strewing quicklime on the surface of any covered place, and letting sheep affected with foot-rot stand on it. "It is not easy," says he, "to defend this practice upon any principle of common sense; for it is impossible but that the indiscriminate application of so powerful a caustic must be attended with serious ill consequences, especially if the feet be not *most accurately* pared, and all the loose horn removed; for otherwise the lime will lodge in the loose pockets of horn, and effect an unnecessary destruction of the quick." Mr. P. may be right: but this has been rather a favourite remedy of ours; and the more so, as we never observed any *bad*, but on the contrary fancied we have seen many *good*, effects from its adoption. We ought, however, to make known, that we have always practised and recommended *careful* and *close paring* of the hoof; not forgetting frequent washing of the part affected with warm milk and water. We perfectly agree with our author in prohibiting all greasy applications, notwithstanding the high authority of Mr. Hogg himself, to whose judgment, either as a shepherd or a poet, we are disposed to pay all due honour. To conclude: our readers may form some idea of the nature of the work by the faint outline we have given above. The substance may be condensed in few words. Foot-rot is induced by *cold* and *wet*. As soon therefore as the sheep are seen to limp, remove them to *warmer* and *drier* situations. If this does not *prevent* the disorder, but it should proceed to the *second* or *suppurative* stage, apply the knife, and butter of antimony, as above directed, as a *cure*: and above all, be careful to keep the feet of the animal as *dry* and as *clean* as possible. A flock thus treated will occasion but little trouble, and very trifling loss.

A Natural Arrangement of British Plants, according to their relations to each other, as pointed out by Jussieu, De Candolle, Brown, &c.; including those cultivated for Use, with their Characters, Differences, Synonyms, Places of Growth, Time of Flowering, and Sketch of their Uses; with an Introduction to Botany, in which the Terms are explained. By Samuel Frederick Gray, Lecturer on Botany, the Materia Medica, &c. In two very large volumes 8vo. with 21 Plates. 2*l*. 2*s*. or with the plates coloured, 2*l*. 12*s*. 6*d*.

A Practical Treatise on Diseases of the Heart.—By Henry Reeder, M.D. Physician to the South London Dispensary, &c. &c.

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ANALYSIS OF PERIODICAL WORKS ON ZOOLOGY AND BOTANY.

A beautiful work on the Land and Fresh-water Mollusca of Germany, by M. C. Pfeiffer, has just come to our hands. It forms a thin quarto volume, printed at Cassel; and is illustrated by eight plates, containing numerous figures, superior in execution to any thing we have yet seen from the German press. It will be regretted however by the generality of our countrymen, that the work is in German, excepting the specific characters, which are given in Latin. Much new matter is also introduced regarding the inhabitant animals; and the figures illustrating this part of the work are truly valuable. We are sorry that our limits will not allow us to notice more than the specific characters of the new species contained in this volume; but it includes such a large proportion of subjects found likewise in our own country, that it becomes an almost necessary addition to the library of every British conchologist.

In the arrangement of families, the author has followed the great Cuvier; the genera accord with those of Lamarck, Müller and Drapanaud; a new one is, however, proposed under the name of *Pisidium*, composed of *Cyclas obliqua* Lam. (*Tellina amnica* Gm.) and *Cy. obtusalis* and *fontinalis* of the same author, principally because they differ from the rest of this group in being inequilateral shells,—a character so trivial, that we are not disposed to concur in the propriety of such a generic distinction.

The species in this volume which the author considers as new acquisitions, are distinguished by the following characters:—

Helix depilata. H. testa subglobosa, perforata, subcarinata, cornea, pellucida, substriata, nitidula; apertura semilunari, peristomate submarginato.

Pupa bidentata. P. testa dextrorsa, cylindrico-ovata, obtusa; apertura bidentata.

Clausilia biplicata. Cl. testa subventricosa, cornea, striata; apertura pyriforme; columella bilamellata; plicis interlamellaribus duabus tribusve.

Clausilia gracilis. Cl. testa fusiformi, gracili, striatula; columellæ lamellis obsoletis. For this species a reference is given to Lister *Hist. Conchyl. lib.* 1. *pars* i. *n.* 39, *f.* b.

Clausilia obtusa. Cl. testa fusiformi, subventricosa, obtusiuscula, striata; apertura ovata; peristomate superius columellæ appresso.

Clausilia minima. Cl. testa cylindrico-fusiformi, læviuscula; apertura ovata; columella faucibusque uniplicatis.

Carychium menkeanum. C. testa conico-oblonga, obtusa, solida, lævi, nitida; apertura oblique pyriformi, 5-6-dentata.

VALVATA. A genus formed by Müller, and of which the type is *Helix fascicularis* of Gmelin. The following is given as a new species:

Valvata depressa. V. testa turbinata, umbilicata; spira depressa, obtusa; apertura circinata, patula.

Among the bivalves, M. Pfeiffer has attempted a further illustration of the genera *Anodonta* (*Anodon* Sw.) and *Unio.* Like all others who have preceded him, this author has drawn the specific distinctions in this intricate group principally from their size and contour. To those conchologists who have formed extensive series of these shells, it will be almost unnecessary to point out the fallacy of making species from such characters; because they are, above all others, the most vague and inconstant; so much so, that the shells from one pond seldom agree perfectly in their form with those of another, and thus species might be made *ad infinitum.* Mr. Swainson appears aware of this, and has directed our attention to the form of the lamellar plate to which the ligament is attached; the modifications of which, he conjectures, will prove the best discrimination of the species. This is a new view of the subject, and certainly deserves investigation. Of the four species enumerated in this work, we consider the *Anodonta cellensis* only as a variety (with a somewhat narrower shell) of *A. cygnea.* The *Anodonta anatina* may probably be a thick variety of our British species; and the *Anodonta intermedia,* we have Mr. Swainson's authority for saying, is the same as his *Anodon pictus,* described in the Bligh collection. Lamarck's *Anodonta intermedia* is compounded of two species. This is shown by his synonyms, one of which refers to *Chemnitz* 8. *tab.* 86. *f.* 763, representing the young of *A. anatinus;* and the other to the *Ency. Méth. pl.* 201. *f.* 2., which is Mr. Swainson's *A. pictus,* and is a continental species. These erroneous synonyms of Lamarck have likewise been repeated in this work.

Among the Uniones, some mistakes also occur: the Linnæan *Pictorum* is given under the name of *Unio rostrata,* after Lamarck; while *ovata* is mistaken for *Pictorum.* On the other hand, *Unio margaritifera* is so well characterized, that we shall give the specific definition; and also that of another, described as a new species, and which we think is really so, being intermediate between *ovata,* and the real *batava,* of the *continental* but not of the *English* writers.

Unio margaritifera. U. testa elliptica, crassa, nigricante; natibus subdepressis, decorticatis; dente cardinali minuto, subconico (obtuso, Ed.) laterali nullo. The true character of this species is here for the first time defined; it consists in the lateral teeth being nearly obsolete, a character overlooked by all previous writers.

Unio riparia. Sp. nov. U. testa elliptica, crassa, fusca; natibus depressis, detritis; cardinis dente conico, crenato.—Gualt. *Ind. Test. tab.* 7.*f. D?* *Ency. Méth.* 249.*f.* 4.

Neither of these figures appears to give a good idea of the same shell as represented by the author at pl. 45. f. 13; particularly that of Gualtieri, which might as well pass for any other species.

Unio litoralis et *batava*, two common species frequent in most parts of the continent, complete the number of this genus; but neither of these is found in Great Britain.

Having now given our readers a short abstract of the contents of this valuable addition to zoological science, we recommend it to their further attention; a few copies have been imported by Mr. Wood, 428, Strand. W. B.

Swainson's Zoological Illustrations. No. 24.

Three plates only are given in this number, occasioned, we presume, by the additional letter-press composing the different indexes and title-pages of the second volume, which this number completes. These plates are however more rich in figures than usual. The first contains two birds, which we have been accustomed to consider as totally distinct; the *Sylvia cayana*, and the *Sylvia cyanocephala* of most writers. But Mr. Swainson appears to have ascertained that they are male and female of the same identical species, and has referred them to their proper station in the system, under the name of *Nectarinia cyanocephala*. Pl. 118: *Conus generalis*; a well-known shell, but enriched by five figures of the different varieties, not well represented by authors.

Ampullaria globosa; a species thus defined:—A. testâ globosâ, lævissimâ, olivaceâ; spirâ depressâ; aperturæ margine crasso, fulvo, sulcato; umbilico parvo, contracto, juxta basin posito; operculo testaceo.

The observations on the *Ampullariæ* are here concluded; and Mr. S. has satisfactorily shown, that the characters given to this genus by Lamarck, are perfectly correct.

We should like to see a larger portion of this valuable work devoted to Entomology: this might be done by curtailing the Shells in this publication, and bringing them in some other work exclusively on conchology.

Sowerby's Mineral Conchology. No. 64.

Pl. 366. *Bulimus costellatus.* We are somewhat surprised at seeing this shell referred to the *Bulimi*. It evidently belongs to the fluviatic genus *Melania*; and the circumstance of its being found in the fresh-water formation of the Isle of Wight, appears to us conclusive on this subject.

Pl. 367 contains *Trochus nodulosus* of Brander, given under the name of *T. monilifer*. Mr. S. observes that Lamarck's *T. monilifer* is distinct from Brander's *nodulosus*, which last is the shell here figured. It would therefore have made the subject somewhat clearer, if Brander's and not Lamarck's name had been followed; because, although Lamarck considered both these shells the same, it would appear he is in error. Brander's original name, therefore, should have been preserved. Nevertheless, the four figures on the plate are so characteristic of the species, that little difficulty will arise hereafter in ascertaining to which species they really belong.

Pl. 368. *Gryphæa bullata*: a new species from the clunch clay near Horncastle in Lincolnshire, approaching very near to *Ostrea*, but more closely to the genus under which Mr. Sowerby has placed it. *Gryphæa vesiculosa* on the next plate occurs near Warminster, and Pl. 370 contains *Pecten asper*; both from the green sand. Pl. 371 is *Pecten cinctus*, found in the alluvial clay of Suffolk, &c.

Sowerby's Genera of Shells. No. 8.

The deficiency of letter-press in the last number is supplied in this; which likewise contains the following additional genera, *Glycimeris*, *Lithotrya*, *Anostoma*, *Crenatula* and *Perna*;—of which we shall give a more detailed account in our next month's Number.

Botanical Register. No. 91.

Pl. 648. *Ixora cuneifolia* of Rox. MSS., allied to *I. parviflora* of Vahl Sym. 3. *tab.* 52, from the East Indies. Pl. 649. *Clerodendron squamatum.* Vahl (*Volkameria Kæmpferi. Willd. Sp. Pl.* 3. 385.) a magnificent shrub; which, from its size and beauty, would have been better figured on a double-sized plate. Native of Japan.

Pl. 650. *Glycine sinensis*: a very elegant climber from China, charmingly represented on a quarto plate.

Pl. 651. *Pyrus coronaria*, of Linn. and Willd.: a beautiful crab-tree, abounding in the woods of North America.

Pl. 652. *Raphiolepis salicifolia*. This appears to have been first noticed as a distinct species by Mr. Lindley: it is likewise a native of China. The characters are,—R. foliis elongato-lanceolatis, paniculâ subcorymboso-fastigiante, petalis dentes calycis æquantibus, staminibus coarctatis calyce aliquantum brevioribus.

Pl. 653. *Psidium polycarpon*, the Guava of Trinidad: an addition to this genus first described by Mr. Lambert in the Linn. Trans. The species we apprehend will be still further increased: our own herbarium contains four or five others hitherto unrecorded by any botanist.

Pl. 654. *Actinotus Helianthi* : a New Holland plant, the *Eriocalia major* of Exotic Botany, *tab.* 78.

Curtis's Botanical Magazine. No. 428.

Pl. 2342. *Argemone albiflora*, placed as a variety of *mexicana* by M. De Candolle; but considered by Horneman and Dr. Sims as a distinct species, which idea is most probably correct.

The next plate represents a very extraordinary, rare, and interesting plant, the *Stapelia tuberosa* of Meerburg; but which Mr. Brown considers as forming a distinct genus, under the name of *Brachystelma tuberosa;* assigning to it these characters: —*Asclepiadeæ.* Corolla campanulata, sinubus angulatis. Columna inclusa. Corona monophylla, 5-fida: lobis antheris oppositis, dorso simplicibus. Antheræ absque membrana apiculari. Massæ pollinis erectæ, basi insertæ.—Inhabits the Cape of Good Hope.

Pl. 2344. The orange variety of *Papaver nudicaule*, from Dahuria: following this, is given *Orobus hirsutus*, from the Levant.

Pl. 2346 and 7. *Lysimachia Euphemerum*, *Phyteuma spicatum;* two plants long known to botanists.

Pl. 2348, *Erica mutabilis* of Hortus Kewensis: and Pl. 2349, *Anchusa Barrelieri*, completes this number. We must beg leave to suggest the great improvement and additional value which would result to this work, if botanical dissections were introduced less sparingly. Of the eight plates in this number, only one contains these illustrating outlines, so essential, not only to the experienced botanist, but to the student. Since botanical science has undergone so many changes, and innumerable genera formed, which a few years back were quite unknown, every assistance of this kind becomes doubly necessary to the elucidation of the subject.

Geraniaceæ. No. 33.

Not a single genuine species of these metamorphosed plants is contained in this month's number.

Loddiges' Botanical Cabinet. Part 65.

Several interesting and singular plants occur in this number; but, in the absence of specific characters or synonyms, we can only notice them by the names here given. *Aristolochia tomentosa:*—under this subject are the following well applied and pious reflections: they evince a pure, a contented, and therefore a happy spirit in the writer; and we join most cordially with him in believing, that if a decided partiality for this beautiful and fascinating study were imbibed in the days of our youth, follies, sanctioned by fashion and example, would lose much of their influence on our future lives. " The singular form of this flower claims our particular attention. Indeed, the same may be said in a greater or less degree of every thing which God has made; and sure it is, that our happiness would be unspeakably promoted by a more habitual contemplation of such things. They are the productions of our own almighty Creator, Preserver, and Benefactor, and we may, at least in an imperfect and feeble manner, behold His glory in the glory of His works."

Templetonia glauca, *Epidendrum nutans*, and *Podalyria styracifolia*, are three other plants remarkable for the singularity of their blossoms.

The First Part of Dr. Hooker's *Exotic Flora* appeared during the last month, and we hasten to lay before our readers a short account of its interesting contents. This publication is designed to present such figures and descriptions of the parts of fructification as may render the knowledge of the genera, and the natural orders of plants, more easy to the botanical student; for which purpose, such plants are as much as possible selected as are worthy of notice from their novelty, rarity, history or uses, or some peculiar or little understood characteristic in their flowers and fruit. With the view to render this work extensively useful, an octavo size has been preferred, with the engravings plain; nevertheless there are a few copies published which have the plates beautifully coloured. It is to appear in parts, of 20 plates each, every three months.

The Part, which is now published, commences with *Tab.* 1. *Caladium Seguinum* (*Arum*, Linnæus), the *Dumb Cane* of the West Indies. Here we have a highly finished, but reduced, figure of the whole plant, with the spadix and spatha of the natural size, and the flowers magnified: the descriptive part consists of the class and order of the individual, its generic and specific characters, in Latin; and a few only of the most important synonyms, (so that the page is not burthened with what more properly finds place in the works of Willdenow and De Candolle); then follows an ample description of the species; and, lastly, whatever is interesting in the history of the plant, with references to the figures. This plan of the letter-press is adopted throughout.—No. 2 is the *Rhipsalis Cassutha*, a genus which the author has separated, though not, apparently, without hesitation, from *Cactus*.—Tab. 3, 4, *Neottia speciosa*, with far more ample details of the inflorescence than have yet appeared, and which are, we think, of the highest importance among Orchideous plants.—No. 5, *Aspidium Wallichii*, a charming and very curious new Fern from Nepal, and thus characterized:—"Frondibus simplicibus lineari-lanceolatis, soris rachis utrinque per totam fere longitudinem lineatim dispositis, stipite inarticulato." Tab. 6. (a folded plate, and thus counting as two), a new *Dorstenia* (*D. arifolia*), native of the Brazils; its specific character runs thus: "Foliis profunde quinquefidis digitato-palmatis, laciniis lanceolatis (junioribus cordato-sagittatis integris), receptaculo elliptico-quadrato inclinato lateraliter pedunculo affixo."—Tab. 7. *Lycopodium dendroideum*.—Tab. 8. *Doodia aspera*, a New-Holland Fern.—Tab. 9. A magnificent East Indian Orchideous plant (on a double plate), *Dendrobium Pierardi*, which flowered for the first time in the Liverpool garden.—Tab. 10. A beautiful *Ophrys*, recently introduced into the gardens of Britain, *O. lutea*.—Tab. 11. *Serapias Lingua*.—Tab. 12. *Calypso borealis*, the American variety, or species as it is considered by Mr. Brown.—Tab. 13. *Sarracenia rubra*; sent, we observe, from Liverpool.—Tab. 14. *Berberis heterophylla*, a native of the Straits of Magellan.—Tab. 15. *Ageratum conyzoides*.—Tab. 16. A novel species of *Pinguicula*, or Butterwort, *P. edentula*: "nectario subulato recurvo, corolla campanulata breviore, quinquelobo, lobis emarginatis integerrimis, palato prominente, scapo pubescente;" and Tab. 17. *Begonia humilis*.

What adds greatly to the interest of this publication is, that most of the plants which it will contain are the produce of the gardens of Scotland, and every part of the work, (except the colouring of the plates, which is under the direction of Mr. Graves of London,) even the engravings, which are of a very superior stamp, is executed in that kingdom; so that we cannot help congratulating our sister country on producing a work of science, which, in point of the excellence of its performance, is, if not superior, certainly fully equal, to any thing of the same kind that has ever appeared in the metropolis of the land.

Botany appears to be making rapid advances in the kingdom of Scotland, if we may be allowed to judge by the productions of its press. At the same time that we announce Professor Hooker's work on *Exotic Plants*, we have the pleasure of recommending to our readers a publication of even greater interest to the student of Cryptogamic Botany, in the *Scottish Cryptogamic Flora* of Mr. Greville of Edinburgh, which is destined to comprise "coloured figures and descriptions of the Cryptogamic Plants found in Scotland, and chiefly of such as belong to the order *Fungi*," and is intended to serve as a continuation of *English Botany*.

To the British botanist we can scarcely think that a more welcome publication could be offered. We have, in the *Flora Britannica* and *English Botany* of the learned and estimable President of the Linnæan Society, a mine of knowledge and information in every department of our Flora, with the exception of what relates to the *Fungi*. These have been in a measure illustrated by the numerous figures of Mr. Sowerby. But in that publication the number of species is very limited, compared with what we now know, and these require to be thrown into a proper arrangement. Persoon, Decandolle, Neis von Esenbeck, and above all the able Fries, have done much towards a systematic distribution of this difficult tribe. It is left for our countryman Mr. Greville (who unites to a classical knowledge of his subject, a power of delineating the object of his pursuit such as is rarely possessed by men of science,) to publish the Fungi of our islands; for we can hardly believe that he will wholly confine himself to the productions of the northern part of Great Britain; and the labour of doing this could not apparently have fallen into better hands.

The *Scottish Cryptogamia* is to appear in monthly numbers of 5 plates each. The engravings are well executed, and the whole got up very much in the same style as the *Flora Exotica* of Dr. Hooker. Mr. Greville has given, we see, a full list of synonyms, and a translation both of the generic and specific characters. The author has established several new orders out of the original *Fungi* of Linnæus and Jussieu, of the propriety of doing which we shall be better able to judge when the work shall be further advanced.

No. 1. contains *Idcrotium durum*, (Persoon); *Agaricus floccosus*, (Curtis); *Isaria microscopica*, a curious new species, parasitical on the *Trichia clavata*, "extremely minute, scattered, solitary, simple, club-shaped, very white, the minute filaments and sporules very indistinct;" *Æcidium Thalictri*, a new species, found by one of Dr. Hooker's students when upon an excursion to Ben Lomond, and thus defined, "growing on the under side of the leaf, somewhat clustered, clusters of a roundish form; peridia oblongo-cylindrical, bright orange, the mouth paler and bursting irregularly;" and *Peziza ochracea*, new species, of which the character stands thus: "minute, sessile, fleshy, thick, yellowish-brown, plane or subconvex, smooth beneath; hymenium (or fructifying surface) sprinkled with granular shining particles."

No. 2. *Sphæria spermoides*, (Hoffmann); *Æcidium Pini*, (Persoon) the largest, as it is probably the most rare of this curious genus; *Uredo Geranii*, (Decandolle); *Agaricus turgidus*, a new species, thus defined: its "pileus somewhat plane, at length convex, very smooth, greyish-brown; gills narrow, numerous, pale; stem large, remarkably hollow, ventricose;" and *Fusarium tremelloides*, (Tremella Urticæ, *Pers.*)

No. 3. *Peziza plumbea*, a new species, "sessile, minute, gregarious, fleshy, depressed, brownish-olive, with a smooth brownish grey hymenium (fructifying surface or disk)." *Uredo oblongata*, (Link), Cryptosphæria Taxi, (*Sphæria* Taxi of Sowerby), a genus which the author has, we think with great propriety, separated from *Sphæria* by the following character: "Receptaculum O. Sphærulæ duriusculæ sparsæ vel aggregatæ, sub epidermide insidentes, ore nunc depresso nunc elongato, erecto aut inclinato, intus massa gelatinosa sporulifera instructa. Sporulæ semper Enudæ." *Polyporus hispidus*, (Fries), and *Puccinia Rosæ*, (De Candolle).

☞ We trust that this useful work will meet with the encouragement which the labour and patient investigation of its author entitle him to expect from the students and admirers of British Botany.

XLII. *Proceedings of Learned Societies.*

ROYAL ACADEMY OF SCIENCES IN PARIS.

July 1. A REPORT was made on a curious fact of vegetable physiology communicated by M. D'Hombres-Firmas. Vegetables, except a small number, and especially trees, do not flower or bear fruit till after a certain growth; and the leafing precedes the flowering. The observation communicated relates to two fine bunches of lilac, on the ground, which M. Villaret took at first sight for bunches detached from a neighbouring tree, and planted in the ground by children, but which he found to be rooted. The reporter considers this fact as very important.

July 15. M. Cuvier delivered a highly-favourable report in the name of a Commission upon the Memoir of M. Floureus, entitled "Researches relative to Sensibility and Irritability." The object of these researches has been to ascertain by a series of experiments, whether the sensitive and the motive powers reside in the whole nervous system, or each in distinct parts of it. And the interesting result appears to be, that in the nervous system there are two properties essentially distinct: the one, to excite muscular contractions; the other, to receive impressions,

sions. It has been the object of M. Floureus to discover, by isolating, and detaching the several parts of the brain and nervous system, in which of them these properties severally reside. We hope to present our readers with fuller information on this important subject in our next number.

M. Cordier has been nominated to fill the place left vacant in the Section of Mineralogy by the lamented death of M. Haüy: and M. Brongniart has been elected to succeed him as Professor at the Museum.

LINNÆAN SOCIETY OF PARIS.

M. Viellot gave an account of an eagle never yet described, killed in the Forest of Fontainebleau, and which he proposes to call *Aquila fasciata*.

A memoir by M. Perrottet was read on the genus *Artocarpus*, commonly called the Bread Tree, in which the author states that this genus is not well understood, and that it contains several distinct species. He describes but four, which he has fully observed in their native soil; the rest he only enumerates, not having seen them, nor choosing to rely on the correctness of the descriptions which have been given of them upon doubtful authority.

XLIII. *Intelligence and Miscellaneous Articles.*

ARTIFICIAL FORMATION OF THE FORMIC ACID. BY M. DOEBEREINER*.

When tartaric acid or cream of tartar, peroxide of manganese, and water, are put together, and the mixture is heated, a tumultuous action presently manifests itself; a great quantity of carbonic acid is disengaged, and it distills at the same time a liquid acid, which on a slight trial would be taken for acetic acid, but which proves by a more strict examination to be the formic acid. This acid, indeed, mixed with concentrated sulphuric acid, changes at an ordinary temperature into water and oxide of carbon: the nitrate of silver or of mercury converts it into carbonic acid by means of a gentle heat, and the two oxides are reduced to a metallic state. Lastly, it forms combinations with barytes, oxide of lead, and oxide of copper, which possess all the properties of those formed by the formic acid.

The residuum left by the tartaric acid and the peroxide of manganese after their reciprocal action, is a mixture of tartrate and formiate of manganese. These two salts may be separated by means of water, which dissolves the latter only.

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* From the *Annales de Chimie* for July 1822.

If, in the process just described, sulphuric acid be added, the tartaric acid will change wholly into carbonic acid, water, and formic acid, and consequently a greater quantity of the latter will be obtained: the best proportions are—1 part of crystallized tartaric acid; 2½ of peroxide of manganese; 2½ of concentrated sulphuric acid, in two or three times its weight of water.

I presume that in several other processes,—for example, in treating sugar, alcohol, and other vegetable substances, with the nitric acid,—and perhaps even in many plants, formic acid is produced, which chemists will be able to observe in future; it is very possible in many cases, where it was supposed that the acetic acid had been observed, that it was the formic acid; for it is known that Fourcroy, and M. Vauquelin himself took the acid drawn from ants for the acetic acid. By means of the relations which I have discovered between the formic acid and concentrated sulphuric acid, and the soluble salts of silver or of mercury, whether dissolved in water or combined with a base, it may be sufficiently and almost instantly distinguished from the acetic acid; so that in future it will not be easy to confound the one with the other.—(*Annalen der Physik*, lxxi. 107.)

On this the editors of the *Annales de Chimie* observe, "We have hastened to repeat the very interesting experiment of M. Doebereiner, and have obtained exactly the result which he describes."

ELECTRICAL CONDUCTORS FOR SHIPS.

The following account has been published of an invention by Mr. W. S. Harris, of Plymouth, for conveying the electric fluid, by means of a copper conductor fixed in the masts, through the bottom of ships. The experiment has taken place in Plymouth harbour, and completely succeeded, as will be seen from the following details:—

Although the advantages of conductors on land are admitted, yet on shipboard, where the effects of lightning are most to be dreaded, from the inflammability of the materials of the ship, the introduction of electrical conductors has been neglected or injudiciously employed. This, indeed, may in some measure be traced to the difficulty of placing any fixed or contiguous conductor in a situation so liable to change and motion as the mast and rigging of a ship; and consequently the only species of conductor that has been adopted is a chain, or long links of wire, one end of which is designed to be hoisted to the mast head, whilst the other passes over the side of the ship, and communicates with the water; but, independently of its defective construction, from its small dimensions, the inconvenience of being constantly hoisted, and its consequent liability to be injured,

jured, are very obvious. This species of conductor is therefore usually kept packed in a case, and only hoisted on the approach of danger, which it may then be too late fully to avert. To remedy these inconveniencies, Mr. Harris proposes to place in the back of the masts a slip of copper, which is to be continued to the interior or hole of the cap of each mast; consequently, coming into contact with the mast above, the continuity will be preserved without the upper masts being lowered. The conductors of the lower masts are to be continued to the keel, and made to communicate with one or more copper bolts in contact with the exterior copper or the water. It must be clear, therefore, that this arrangement preserves a permanent conductor so long as any part of the mast is continued; and as the masts of a ship may be considered as mere points when contrasted with a thunder cloud, thus armed they are virtually pointed conductors. To those acquainted with the action of points on charged electrics, it will be obvious, and not too much to presume, that such masts will be highly efficacious in silently depriving a thunder cloud of its charge, thereby giving to ships a degree of security of very considerable importance.

From these considerations Mr. Harris was induced to submit a model of a complete mast, furnished with permanent conductors, to the inspection of the Honourable Navy Board, who expressed their decided approbation of the principle, and requested him to exemplify its efficiency by an experiment, which was carried into effect, on Monday September 16, on board the *Caledonia*, at Plymouth, in the presence of the Navy Board, Sir A. Cochrane, Commissioner Shield, several Captains in the Navy, and the principal officers of the Dock-yard, in the following manner:—The *Louisa* cutter having had a temporary mast and topmast fitted with a copper conductor, according to Mr. Harris's plan, was moored astern of the *Caledonia*, and at the distance of eighty feet from the cutter a boat was stationed with a small brass howitzer. On the tiller head of the *Caledonia* were placed the electrical machine and an electrical jar, with the outer coating of which a line was connected, having a metallic wire woven in it: this line being carried out of the starboard window of the wardroom, terminated in an insulated pointed wire in the immediate vicinity of the touch-hole of the howitzer; a similar line was passed from the larboard window, which communicated with the mast head of the cutter: and at the termination of the bolt through the keel, a chain was attached, connected with another insulated pointed wire in the boat, placed in the vicinity of the touch-hole—the space between the insulated points being the only interval in a circuit of about 300 feet, from the positive to the negative side of the jar. Some gunpowder being placed in contact with the conductor in the cutter, and the priming in the interval of the insulated points, the jar was charged, and the line attached to the mast head of the cutter being brought into contact with the positive or inside of the jar, a discharge of electric matter followed, which was passed by the line to the mast head, and by the conductor through the powder to the chain in the water, by which it was conveyed to the interrupted communication in the boat, where it passed in the form of a spark, and discharging the howitzer, returned to the negative or outside of the jar by the line leading into the starboard window, thereby demonstrating that a quantity of electric matter had been passed through the powder (without igniting it) in contact with the mast of the cutter, sufficient to discharge the howitzer. Mr. Harris then detached the communication between the keel of the cutter and the positive wire in the boat, leaving that wire to communicate with the water only; but this interruption did not impede or divert the charge, as the discharge of the howitzer was effected with equal success as in the first instance, the water forming the only conductor from the cutter to the boat. In order to demonstrate that a trifling fracture or interruption in the conductor would not be important, it was cut through with a saw; but this produced no material injury to its conducting power.

These trials, carried on under the disadvantages of unfavourable weather, could not fail of convincing all present of its efficacy, and called forth the decided approbation of the Navy Board in particular, which was evinced by Sir T. B. Martin requesting Mr. Harris to superintend the equipment of the masts of the *Minden*, 74, and *Java* frigate, preparatory to its general introduction into the navy.

BLACK SAND OF CAYENNE.

From a paper by M. Gillet de Laumont on the black sand of Cayenne, we learn: 1. That this sand produces two very distinct varieties of iron, one capable of attraction by the loadstone, the other not: the first contains 10, the second 32 per cent. of oxide of titanium and a small portion of manganese:—2. That melted together these two varieties will give good castings proper for mill-work, or will furnish, when treated like other ferruginous minerals, malleable iron:—3. But that it is important to set on foot experiments on a grand scale on the spot, similar to those announced by M. Berthier in his analysis of titanized iron of Brazil (*Annales des Mines*, tome v. pp. 479 et seq.)

ORNITHOLOGY OF ICELAND.

COPENHAGEN: *Natural History.*—Mr. F. Faber, who resided three years in Iceland, and travelled all over that mountainous island, formed, while there, a large collection of birds and their eggs, which are now in the Royal Museum. He has just published a preliminary notice of his discoveries, in Latin, under the title of A Prodromus of Islandic Ornithology.

NUMBER OF PLANTS CULTIVATED IN BRITAIN.

Since the discovery of the New World, our English gardens have produced 2345 varieties of trees and plants from America, and upwards of 1700 from the Cape of Good Hope, in addition to many thousands which have been brought from China, the East Indies, New Holland, various parts of Africa, Asia, and Europe; until the list of plants now cultivated in this country exceeds 120,000 varieties.

EARTHQUAKE IN THE NORTH OF ENGLAND.

A smart shock of an earthquake was distinctly felt at Dunston, near Newcastle, between one and two in the morning of September the 18th, accompanied by a loud noise like distant thunder. Several of the inhabitants of the village were awakened from their slumbers, and much alarmed by the circumstance of the chairs, tables, and other furniture in their houses being removed; and in one house the head of the clock case was thrown down by the violence of the concussion.—*Durham Chronicle.*

CAPTAIN SCORESBY'S DISCOVERIES IN GREENLAND.

The ship *Baffin*, Captain Scoresby jun., arrived here on Thursday, from Greenland, with 195 tons of blubber, the produce of 9 whales. During the intervals of the fishery, Captain Scoresby employed himself in making observations on the geography and natural history of the long-lost eastern coast of Greenland, which was within sight for three months. The result, we understand, is a survey of the eastern coast of that almost unknown country, from lat. 75. N. to 69., comprising an extent of coast, reckoning its numerous indentations, of about 800 miles. Captain Scoresby discovered some extensive inlets, from the number of which he is induced to consider the whole country a large assemblage of islands. He landed on various parts of the coast, and on each visit to the shore discovered recent traces of inhabitants, and obtained fragments of their implements. It is important to geography, to know that the form of this land surveyed by Captain Scoresby is extremely unlike what it is represented in our best charts, and that the error in longitude, in most cases, was not less than

15 degrees. We understand that he has made large collections of plants and minerals, particularly of geological specimens. The *Baffin* left the coast of Greenland on the 27th of August, soon after encountering a tremendous storm, in which the *Dundee* of London was dismasted.—*Liverpool Advertiser*.

RUSSIAN VOYAGE OF DISCOVERY.

St. Petersburgh, Sept. 6.—Captain Wassiliew, who commanded the two vessels that have just returned from their voyage of discovery, has performed great services to geography. He discovered in the great ocean a group of inhabited islands: passed through Behring's Straits, and reached a higher latitude than Cook; determined the true position of North America, from Icy Cape to the peninsula of Alaska, and found to the north of it another inhabited island.

JUNCTION OF THE AMERICAN LAKES WITH THE ATLANTIC OCEAN.

This magnificent work, calculated to improve prodigiously the commerce of New York, goes on nobly to its completion. In a few months more, by means of this Grand Western Canal, our vessels will pass from the ocean to our inland seas. In executing this work, which does great honour to its projector, nearly ten thousand men are said to be employed. We are also informed that another canal, about forty miles in length, is intended to be cut from Providence to Worcester, which will still further facilitate the transit of goods from New York to the interior of Massachusets.—*American Paper*.

WOOL EMPLOYED IN THE MANUFACTURES OF FRANCE.

"Formerly, and even to the present time, the clothiers sought out for long and sound wools, in the expectation of finding in them proportionally less of grease and foreign matter, and more freedom in the staple, but also with the view of giving more strength and firmness to their cloths. The very reverse now takes place. In order to maintain a competition with the cloths of England, they require only such wools as are short, weak and silky: and they manufacture only fine and slight cloth, soft and silky, which will scarcely last so long as the transient taste which has given rise to them.

"This predilection has produced a change in our flocks; and the grower of the raw material, who seeks for a ready sale, has been compelled to conform to the desires of the manufacturers and the depraved taste of the consumer. In order to diminish the length of their wool, they have been tempted to shear their sheep twice in the year, allowing seven months for the first growth and five for the second: but, besides that it is only a small number which is capable of yielding twice in the year a wool of sufficient length; the prospect of reaping advantage from animals twice in the year submitted to the shears, seems more than doubtful. The experiments made by M. Bourgeois, on his estate at Rambouillet, leave no doubt on this subject."

If the above facts (taken from the *Bibliothèque Physico-Economique*) are to be depended upon, they are worthy the attention of the English clothier, who will perceive that the French clothier has had recourse to a change in the wools consumed in his manufacture, with a professed view of maintaining a rivalry with English cloth.

PALM-TREE CORDAGE.

A specimen of the palm-tree cordage, recently invented in North America, has been sent over to Liverpool from New York. It is very beautiful, and from its appearance much stronger and more elastic than cordage manufactured from hemp.

CHANGE OF COLOUR IN A NEGRO.

An advertisement in the Norfolk Beacon, United States, apprizes the public, that a natural curiosity at that place is now exhibiting. The object is a man of sixty years of age, and of uncommon intelligence, who was born black, and continued so until the age of forty-five; since which, he has gradually undergone a change of skin, until three-fourths of him have become perfectly white, and his arms and hands have assumed a delicacy and transparency not surpassed by those of the most tenderly bred female.

OBITUARY.—SIR WILLIAM HERSCHEL.

This distinguished astronomer died at his house at Slough, on the 26th of August, in the 84th year of his age.

Sir William Herschel was born in November, 1738; his father being a musician, brought up his four sons, of whom Sir William was the second, to the same profession, and placed him, at the age of 14, in the band of the Hanoverian Foot Guards. Unable, however, long to endure the drudgery of such a situation, and conscious of superior proficiency in his art, he determined on quitting the regiment, and seeking his fortune in England, where he arrived about the end of the year 1757. After struggling with great difficulties in London, he was engaged by the Earl of Darlington to superintend and instruct a military band then forming by that Nobleman in the county of Durham, and the opening thus afforded contributed so far to increase his reputation and connexions, as to induce him to spend several years after the termination of this engagement in the neighbourhood of Leeds, Pontefract, Doncaster, &c., where he had many scholars, and led the public concerts, oratorios, &c.

In 1766 he was chosen organist at Halifax, a situation he soon after resigned for the more advantageous one of organist at the Octagon Chapel at Bath. In this great and gay resort of fashion, his extraordinary musical talents procured him ample employment; and the direction of the public Concerts, and his private teaching, produced him a considerable income.

But though fond to enthusiasm of his profession, his ardent thirst for knowledge had begun for some time past to open a nobler field to his exertions. While at Halifax, he had commenced a course of mathematical reading; and in spite of the difficulty of such studies, acquired without assistance a considerable familiarity with the principles both of pure and mixed mathematics. The sublime views disclosed by the modern astronomy had powerfully attracted his attention; and when he read of the noble discoveries made by the assistance of the telescope, he was seized with an irresistible desire to see with his own eyes the wonders he read of. *Fortunately* the price of an instrument capable of satisfying his curiosity was beyond his means, and he resolved to attempt the construction of one for himself. In this arduous task, after encountering endless difficulties, he succeeded, and in 1774 first saw Saturn in a five feet reflecting telescope made by his own hands. Encouraged by this success, he now attempted larger telescopes, and soon completed a seven, a ten, and a twenty feet reflector, labouring with such obstinacy as to have actually finished no less than 200 object mirrors before he could satisfy himself with the performance of one.

Astronomy now occupied so much of his attention, that he began to limit his professional engagements, and restrict the number of his scholars.

About the latter end of 1779, he commenced a regular review of the heavens, star by star, with a seven feet reflector; and having already continued this upwards of 18 months, he was at length rewarded on the 13th of March, 1781, with the discovery of a new primary planet, to which he afterwards gave the name of *Georgium Sidus*, now more generally distinguished by that of *Uranus*.

In consequence of this memorable discovery, the attention of the scientific world became fixed upon him; and His late Majesty, with a promptitude of liberality which must ever be recorded to his honour as a patron of science, enabled him, by the settlement of a handsome salary, to discontinue his professional exertions, and devote the remainder of his life wholly to Astronomy. In consequence of this arrangement, Herschel immediately quitted Bath, and took up his residence at Datchet in the neighbourhood of Windsor, where he was no sooner established than he entered on a career of discovery unexampled, perhaps, in the history of science. Having removed to Slough, he commenced the erection of a telescope of yet larger dimensions than any before attempted, which he completed in 1787, and aided by this stupendous instrument, and by others of hardly inferior power, extended his researches to every part of the heavens, penetrating into regions of space of a remoteness eluding calculation, and developing views of the construction of our own system and the universe, of a daring sublimity, hardly more surprising than the strictness of the induction on which they rest.

In these observations, and the laborious calculations into which they led, he was assisted throughout by his excellent sister, Miss Caroline Herschel, whose indefatigable and unhesitating devotion in the performance of a task usually deemed incompatible with female habits, surpasses all eulogium. His discoveries were communicated as they arose to the Royal Society, and form an important part of the published Transactions of that learned body from the year 1782 to 1818.

In 17—, he married Mary, widow of the late John Pitt, Esq. and the accession of domestic happiness he experienced from this union, while it testified the justness of his choice, contributed powerfully to cherish that calm tranquillity of mind which is the native element of contemplative philosophy, and the soil from which its shoots rise most vigorous and most secure.

In 1816, His present Majesty was graciously pleased to confer on him the decorations of the Guelphic Order of Knighthood. His astronomical observations were continued within a few years of his death, till, his declining strength no longer keeping pace with the activity of his mind, he sunk at length full of years and glory, amidst the applause of the world, and, what was far dearer to him, the veneration of his family, and the esteem and love of all who knew him.

Sir William Herschel has left one son, who, with his father's name, inherits his distinguished talents.

M. Delambre, perpetual Secretary of the Royal Academy of Sciences for the class of Mathematical Sciences, died at Paris, Aug. 27, aged 72. His funeral at the church of the Abbey was attended by a great number of members of the Institute, and other distinguished persons. His remains were deposited in the cemetery of P. La Chaise; and three of his colleagues,—M. Cuvier on the part of the Academy of Sciences, M. Biot in the name of the College of France, and M. Arago in the name of the Bureau of Longitude,—expressed, in three discourses listened to with emotion, the grief which his loss has caused to all friends of science. M. Delambre leaves vacant the place of perpetual Secretary to the Academy, that of Member of the Board of Longitude, and of Professor of Astronomy to the College of France. M. Fourier and M. Biot are mentioned by their friends as successors to the place of Secretary. The election is adjourned till November next.

LECTURES.

Surry Institution.—The following Courses of Lectures will be delivered in the ensuing season:

On the History and Utility of Literary Institutions; by James Jennings, Esq.

On Chemistry; by Goldsworthy Gurney, Esq. in the Course of November.

On Music; by W. Crotch, Mus. D. Professor of Music in the University of Oxford; and,

On Pneumatics and Electricity; by Charles Woodward, Esq. early in 1823.

LIST OF PATENTS FOR NEW INVENTIONS.

To David Mushet, of Coleford, Gloucestershire, iron-maker, for his improvement or improvements in the making or manufacturing of iron from certain slags or cinders produced in the working or making of that metal.—Dated the 20th August 1822.—6 months allowed to enrol specification.

To William Mitchell, of Glasgow, silversmith, for his process, whereby gold and silver plate and other plate formed of ductile metals may be manufactured in a more perfect and expeditious manner than by any process which hath hitherto been employed in such manufacture.—24th August.—6 mo.

To Thomas Sowerby, of Bishopwearmouth, Durham, merchant, for a chain upon a new and improved principle suitable for ships' cables and other purposes.—29th August.—2 mo.

To Robert Vazie, of Chasewater Mine, in the parish of Kenwyn, Cornwall, civil engineer, for his improvement in the compounding of different species of metals.—3d Sept.—6 mo.

To Henry Burgess, of Miles-lane, Cannon-street, London, merchant, for his improvements on wheel carriages.—3d Sept. —6 months.

METEOROLOGICAL TABLE,

The *London* Observations by Mr. Cary, of the Strand.
The *Boston* Observations by Mr. Samuel Veall.

Days of Month. 1822.	Thermometer. London. 8 o'clock Morning.	Thermometer. London. Noon.	Thermometer. London. 11 o'clock Night.	Thermometer. Boston.	Height of the Barom. Inches. Lond.	Height of the Barom. Inches. Boston	Weather. London.	Weather. Boston.
Aug. 27	55	63	57	70	29·74	29·15	Fair	Fine
28	55	66	55	68	·76	29·30	Fair	Cloudy
29	55	67	56	62	·57	29	Fair	Windy
30	54	61	55	69	·86	29·35	Showery	Fine
31	54	67	57	66	·99	29·50	Fair	Fine
○ Sep. 1	54	66	56	68	30·17	29·76	Fair	Fine
2	55	67	59	69·5	·14	29·50	Cloudy	Cloudy
3	58	69	58	63	·01	29·45	Fair	Cloudy
4	55	68	62	62·5	·13	29·65	Fair	Cloudy
5	65	66	62	70·5	29·91	29·35	Cloudy	Cloudy
6	62	67	55	72	·87	29·35	Showery	Fine
7	54	67	55	68·5	30·12	29·63	Fair	Fine
8				68·5		29·35		Windy
9	61	66	59	60	·10	29·60	Fair	Windy
10	62	65	60	59	·12	29·83	Fair	Fine
11	60	68	54	68·5	29·88	29·33	Cloudy	Fine
12	50	58	53	59	30·09	29·65	Showery	Fine
13	54	59	44	59	·10	29·80	Cloudy	Fine
14	50	61	55	61·5	·24	29·92	Cloudy	Fine
☾ 15	55	60	57	61	·10	29·85	Cloudy	Cloudy
16	50	60	60	69	·17	29·70	Fair	Fine
17	55	72	57	70	·11	29·70	Fair	Fine
18				65		29·80		Fine
19	54	62	53	62·5	·20	29·85	Fair	Fine
20	54	62	52	62·5	29·97	29·68	Fair	Fine
21	53	60	52	61	·92	29·60	Fair	Fine
22	56	59	55	60·5	·72	29·68	Showery	Fine
23	57	59	55	60	·80	29·50	Fair	Cloudy
24	56	59	55	59·5	·33	29·05	Showery	Cloudy
25	52	57	54	57	·43	29·20	Showery	Cloudy
26	50	55	51	54·5	·78	29·55	Showery	Fine

N.B. The Barometer's height is taken at one o'clock.

Erratum in last No.:—P. 152, line 15, *for* heat *read* beat.

THE

PHILOSOPHICAL MAGAZINE
AND JOURNAL.

31st *OCTOBER* 1822.

XLIV. *On the proposed Alteration of Weights and Measures, and the Adaptation of Gauging Apparatus thereto.* *By* WM. GUTTERIDGE, *Esq.*

To the Editors of the Philosophical Magazine and Journal.

Saint Fin-barrs, Cork, Aug. 6, 1822.

GENTLEMEN,—AS an advocate for the change contemplated in the weights and measures of the nation, I solicit your permission to address a few observations upon the subject through the medium of your Magazine.

The theory, upon which the alterations are grounded, is as follows:

1. That an inch in length shall be $\frac{10000}{391393}$ parts of the length of a pendulum, vibrating seconds in London, in a vacuum, upon a level of the sea; hence the length of the pendulum, from the axis of suspension to the centre of oscillation, will be $39\frac{1393}{10000}$ inches.

2. That a grain troy shall be equal in weight to $\frac{1000}{252456}$ parts of a cubic inch of distilled water, at 62° of temperature, in air; hence the weight of a cubic inch of such water will be $252\frac{456}{1000}$ grains.

3. That 7000 troy grains shall constitute an avoirdupois pound.

4. That 10 lbs. avoirdupois of such water shall fill a gallon; hence the cubical capacity of the gallon will be $277\frac{276}{1000}$ inches.

5. A gallon of water, between 70° and 39°, varies in bulk rather less than $\frac{1}{3}$ of a cubic inch.

6. Clear river water exceeds distilled as 1·0006 to 1, or about $\frac{1}{6}$ of a cubic inch in a gallon.

7. Taking ·00001044 as a mean expansion of brass for each degree of the thermometer, varies the content of a gallon, between 70° and 39°, just $\frac{1}{3}$ of a cubic inch.

For the accommodation of the public, I have performed all the requisite alterations upon gauging apparatus, to render them exclusively applicable to the new system. Some I have altogether superseded the necessity of, by introducing instruments of my own invention, less operose and more correct. In point of accuracy, I will just give one instance by way of specimen.

An ingenious revenue officer, Mr. J. Overley (by far the most scientific author who ever wrote in England, as to gauging in particular), states that, according to Ward's Young Mathematician's Guide, a spheroidal cask having 31·5 and 24·5 inches for the bung and head diameters respectively, and 42 inches for the length, will contain 100·78 gallons of English ale measure.

In the 103d page of the work, he has shown the method to construct a gauging instrument, called a diagonal-rod.

He states that "this line is constructed for a cask whose content is 60 ale gallons, and the diagonal thereof 30 inches; therefore the proposition holds:

"As 60 is to the cube of 30, so is any other content to the cube of the diagonal."

Now, gentlemen, I shall undertake to prove that those casks were not both the *frusta* of the spheroid; and, as that is the sort of cask from which all gaugers draw their conclusions, that the diagonal rod is very considerably in error. My letter in your Magazine for August 1818 furnishes me with abundant reason for scepticism; and I am aware the author took this matter upon trust from Oughtread, who indeed was no mean authority. The theory is perfectly true if applied to Ward's cask. The object of this rod is to ascertain the content of any cask. The operation is as follows:—Through the orifice in the middle of the staff pass the rod, until it meet the intersection of the opposite staff and the head; then just in the centre of the orifice will stand the content of the cask, upon the rod, among a scale of unequal graduations engraven upon it; which graduations result from a tarif formed from the analogy before given; and since homologous solids are to one another as the cubes of their dimensions, we have a method of proving the justice of my disapproval of the rods now used, which I discover to have been uniformly constructed from the erroneous cask selected by Oughtread; if, indeed, he ever applied to a cask at all; which I have abundant reason to suspect he never did. For it could not have been either of the *first*, *second*, or *third variety;* but rather the *frusta* of two cones abutting upon one common base; and which is such a cask as is only known in theory.

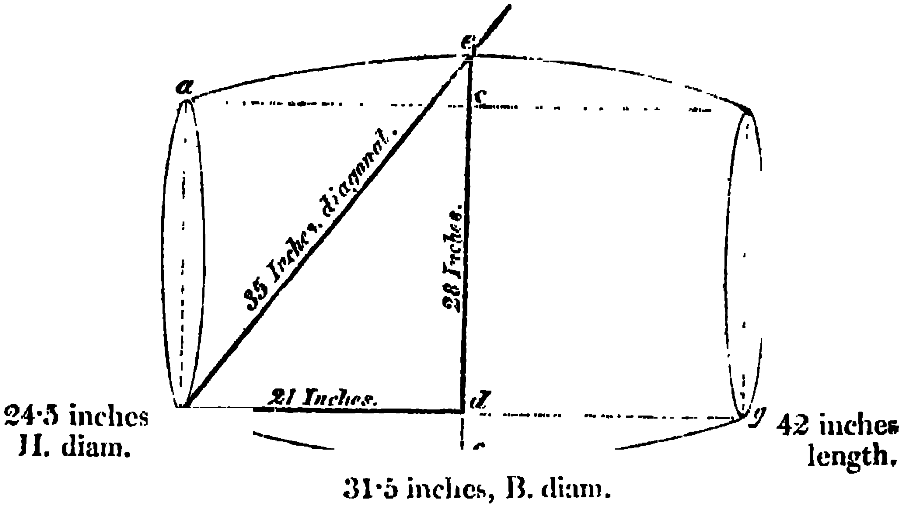
The annexed engraving will be explanatory of the subsequent explications, where

The length $= bg =$ 42 inches.

The bung diam. $= ef =$ 31·5 inches.

The head diam. $= ab =$ 24·5 inches.

The half length $= bd =$ 21 inches, or base of a triangle, of



of which the hypoth. or diag. *eb* will be 35 inches, and the perp. *ed* will be 28 inches, as we will prove thus:

ab and *cd* are respectively equal to one another; and *ec* and *df* are also equal to one another: therefore, if *ec* be added to *ab*, the sum will be *cd*. Or, *ec* is half the difference between *ab* and *ef*, consequently *ed* is the arithmetical mean between 31·5 and 24·5, and $\frac{24{\cdot}5+31{\cdot}5}{2} = 28 = ed$; and since 21 and 28 are the two sides of a right angled triangle, including the right angle, the diagonal *eb* will, according to the 47th prop. 1st Euclid, be $\sqrt{\overline{28}|^2+\overline{21}|^2} = 35$; for the sum of the squares of 28 and 21 make 1225, to extract which by logarithms we have

1225, its log. =2)3·0881361

its root 35, its log. = 1·5440680; therefore,

100·78 ale gallons ought to be found upon the rod, among the engravings, at the 35th inch; instead of which, if we follow Overley, we have

galls. diag. galls. diag.

As 60 : $\overline{30}|^3$ :: 100·78 : $\sqrt[3]{45351}$ = 35·66 instead of 35; for by logarithms

$\sqrt[3]{45351}$, its log. = 3)4·6565869

its root 35·66, its log. = 1·5521956; and from this analogy the diagonal rods, now in use, are constructed. We will now see what the diagonal of this 60 gallon cask ought to have been instead of 30 inches, according to Overley: thus,

galls. diag. galls. inches.

As 100·78 : $\overline{35}|^3$ :: 60 : $\sqrt[3]{25525{\cdot}898995}$ = 29·45.

For 25525·898995, its log. 3)4·4069810

its root 29·45, its log. = 1·4689937, which is different from that stated by Overley by above half an inch. My rod is constructed from a tarif of cube roots, resulting from the preceding analogy, with the exception of the number of gallons, which were an equivalent of *imperial measure* to the foregoing quantity of ale measure.

I am, however, free to confess that this error for a long time escaped my notice; and I think it very probable, that if I had not determined upon entirely new modelling of every species of gauging instruments, to render them exclusively applicable to the new system, I never might have discovered it; but, having been true to my purpose, I have the satisfaction of being enabled to state that my improvements will form a valuable appendage to the imperial system; to which it so readily applies in practice. It may not be improper to exhibit the ratio of loss which this improvement obviates.

This will be shown by inverting the analogy; thus,

	inches.	ale galls.	inches.	ale galls.	
As	$\overline{35}\|^{3}$:	100·78 ::	$\overline{35·66}\|^{3}$:	106·5894	from
	true diag.	content of the cask.	erroneous diag.	100·78	take
				5·8094	loss or error.

Here the loss to *two* of the *three parties concerned* is shown to be above 5½ per cent., the revenue being at the *minus* side of the question.

As manufacturers are mere copyists, it is no wonder that they should know nothing of the matter. This rod, I may observe, is in use, more or less, all over the nation; but it is due to the port gaugers to state that they do not use it. It ought to be strictly prohibited, or indeed destroyed; at any rate, especial care should be taken that no rod of this sort should be used for the imperial measure, until it has been ascertained not to have been derived from Oughtread's analogy.

The port gaugers, as I observed before, do not use this rod. They use the callipers. The callipers are however not without defect.

They give the *external* dimensions; and although the great experience of the port gaugers enables them to excel in their profession, I must consider that instrument objectionable which does not *direct*, and not *derive from*, the judgement. The process of perforating the head of a lying cask, to ascertain the thickness of the timber, is inadmissible; but the loss of time, even if practicable, would forbid its adoption in every cask. To obviate this objection, I have invented an instrument to ascertain, promptly, the *length* of any cask from an *interior application* from the bung, and which *at the same instant* exhibits the *bung diameter*.

But my object is not to give more than a cursory account of a few of the most singular of my inventions and improvements; and therefore I shall only glance at one more new invention, which, from its singular utility in carrying into effect the imperial system, deserves particular notice. It being altogether a novelty, I scarcely know by what name to call it. The instrument which ascertains the *interior* length of a cask, I term an *interiometer*, in contradistinction to an instrument for inching of vessels from *without*, which I term an *exteriometer:* but as this is for taking of dimensions generally, I sometimes call it an *imperial meter;* because it is to ascertain the number of *gallons or bushels imperial, from the mere involution of its dimensions.* I sometimes think it ought to be termed *an imperial cubit;* but as names are of no importance, I will show the utility. This will be obvious from the nature of a simple question and answer; thus,

Quere.—1. A vessel being 5·6 *meters* in length, 4 *meters* in breadth, and ·5 meter in depth, I demand the number of *imperial gallons* which it would contain?

Solution. 5·6 *meters*, multiplied by
4 *meters.*

22·4 = product of length and breadth.
·5

Gallons contained 11·20 = product of length, breadth and depth.

Quere.—2. A cylinder being 5·6 *meters* in diameter, and 4·2 *meters* in depth, I demand the number of imperial gallons which it would contain?

Solution. 5·6 *meters* or diameter squared.
5·6

336
280

31·36 = square of diameter.
4·2 = meter's depth.

6272
12544

Gallons contained 131·712 product of depth and squared diameter.

If the question had been *bushels*, the process would have been just the same; but a *bushel meter* is exactly twice the length of a *gallon meter:* on one edge of a rod are therefore engraven a series of *gallon meters,* and upon the opposite edge a series of *bushel meters*;—1, 2, 3, upon the latter standing against 2, 4, 6, upon the former.

But in ascertaining the content of cylinders, either in *bushels* or *gallons,* the *meters* consequently of the *depths* of cylinders are different in their length from the *meters* used for the *diameters,* and for the dimensions of angled figures. In consequence of these differences in the lengths of *square* and *circular meters,* the *former* are engraven upon one side of the rod, and the *latter* upon the other side. And there is the same uniformity between the *circular meters* for *bushels* and *gallons* as exists between the *square meter,* as before explained, as 2 to 1.

And although the superiority of this invention over all others for ascertaining dimensions in terms the most readily introduced into practice, to give the final result, by the pen, will be readily granted; yet I have peculiar satisfaction in being enabled to state, that its greatest advantage is its not requiring computation at all, if the sliding rule be used. And an additional advantage is, that the content for *bushel* or *gallon* and from *square* or *circular meter* is ascertained by one general operation. It requires no gauge point upon the rule whatever, if an inverted line of numbers be substituted for the ordinary line D; or if the latter line be retained, the gauge point for *bushel* or *gallon* from *square* or *circular* figures, will be the same in all cases; but, except for ullaging, a one-slide rule is all that is requisite; therefore the advantages are manifold of which this invention is chief. Sliding rules, best suited to the purpose, I have also invented, to accompany the *meter,* that nothing should be deficient. I will just state, that however important my improvements may be in facilitating different commercial transactions, I think that if the Legislature were to pass a law to compel people to adhere to the just and equitable mode of applying those measures, so that the conclusions might be conformable to common sense, it would be no more than the interests of many of the legislators, as well as thousands besides them, render most imperative. This letter would be too long if I were to enter into an history of the *tolerated barbarous perversions of true measures in our nation!*

Allow me, gentlemen, to add that, if you will permit me, *I will prove,* in your next Magazine, *that, such is the extent of this evil, a most valuable species of property is depreciated above* 20 *per cent.* I will not only prove that such is the case, but I will also submit a rational theory which shall be capable of most effectually obviating such malpractices in future. I take this opportunity to express my entire concurrence with the two clauses in the Act for altering of the present weights and measures, which require that tables of equalization and conversion from the quantities and values of present measures to that of the imperial measure, shall be prepared and published.

I was so thoroughly convinced of the necessity of this, that *I actually prepared tables of equalization and conversion, as to quantity and value, from all the multifarious measures in the British empire to that of the imperial standard.* And as the Act is put off until next Session, I mean then to be also prepared with similar tables to convert from all *foreign standards* to the same standard; thoroughly convinced that, if I am any way seconded in my efforts, I shall be enabled to give to the world one of the greatest advantages, in a commercial point of view, which has ever been submitted to the public. And as I doubt not that truth and equity are the prominent characteristics of your publication, and that you will readily admit the necessity of accuracy, in a great commercial nation like ours, in matters involving the best interests of thousands of its inhabitants, I am persuaded that you will permit me to endeavour, in your next, to obviate the evils of which I complain.

I have the honour to be, gentlemen,
Your most obedient humble servant,
Wm. Gutteridge.

XLV. *On an Improvement in the Apparatus for procuring Potassium.* By William Mandell, *B.D. Fellow of Queen's College.*[*]

On repeating the late Professor Tennant's experiment for procuring potassium[†], (which differs from the similar one first made by the French chemists, Gay-Lussac and Thenard[‡], principally in being more simple and commodious for practice,) it occurred to me, that one part of the apparatus made use of, might, with advantage, be still further simplified: and as every circumstance, however apparently obvious or trivial in itself, which, in any degree, tends to facilitate the production, in greater quantity, of so powerful a chemical agent as potassium, is of importance, I have thought that the mode of operating which I pursued might not be wholly unworthy the notice of this Society.

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[*] From the Cambridge Philosophical Transactions for 1822, Part II.

[†] Philosophical Transactions for 1814, p. 578, to which the reader is referred for the detail of the process. See Phil. Mag. vol. xliii. 457.

[‡] *Annales de Chimie*, tom. lxvi. p. 205.

It is well known that the grand difficulty in successfully performing the experiment in question, consists in protecting the gun-barrel from the effects of that extreme and long-continued heat, which is necessary to decompound the alkali, and to volatilize its base. The usual practice hitherto has been to surround with a *lute*[*], that portion of the gun-barrel which is introduced into the fire. This operation, however, is always tedious; and although it be conducted even with the greatest care, it is found extremely difficult to prevent fissures in the coating, particularly when the heat is much increased in the course of the experiment. Hence, if eventually the fire have direct access to the barrel, through any crevice which may be formed, the fusion of the denuded part is generally the consequence, and the whole labour of the experiment is lost.

This, then, being the common cause of failure, it occurred to me that, if there were substituted for the luting, a thin but sound and well-burnt *tube* or *hollow cylinder* of Stourbridge clay, of such dimensions as just to cover that portion of the barrel which is subjected to the fire, the unfortunate result, which I have alluded to, might possibly be avoided.

A tube of this description was accordingly procured; and, in order to guard against the hazard of its cracking, by reason of exposure to a sudden increase of temperature, it was, in the first place, gradually and with caution, heated to redness.

The remaining part of the experiment was then performed with entire success; and a very considerable quantity of potassium obtained.

It may be proper to remark that the hollow cylinder, and that portion of the gun-barrel which it incloses, should be of such relative diameters that, when cool, their corresponding surfaces are not quite in close contact; otherwise, the cylinder will be in danger of bursting, not only on account of its own contraction, but also on account of the simultaneous expansion of the gun-barrel, from the effects of that very high temperature, to which, in this state of combination, they are submitted.

Moreover, the whole apparatus should be supported accurately in the same position, throughout the experiment, (by means of rests made of Stourbridge clay,) and should be so situate in the fire, that the materials operated upon may during the whole process be subjected to its greatest intensity.

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[*] "On couvre cette partie moyenne d'un *lut* infusible."—Gay-Lussac et Thenard, *Ann. de Chim.* tom. lxvi. p. 207.

"The *lute* which I have found most effectual ...... was composed ...... of Stourbridge clay ...... &c."—Tennant, Phil. Trans. for 1814, p. 582.

"It (the gun-barrel) is then covered with an infusible *lute*."—Brande's Manual of Chemistry, p. 184. Ed. 1819.

With due attention to these precautions, and to some minor circumstances in the manipulation of the experiment, which I shall not take up the Society's time in detailing, it is believed that the decomposition of potash, by means of iron, might in every instance be effected with almost entire certainty, and potassium be obtained in great abundance.

XLVI. *On the Origin and Discovery of Iron.* By DAVID MUSHET, *Esq.*[*]

[Continued from page 167.]

HOWEVER rude an instrument, in the hands of the early iron-maker, we may in our times consider the blast-bloomery to have been, yet there can be no doubt that in an operation so different from, and so much more complete than, the more ancient air-bloomery, many difficulties attended its general introduction. Prior to this, bellows must have been invented and in common use, and their construction substantial and well understood, before they were made powerful enough to smelt ores of iron. It is also very probable that they were long used to forge the iron produced in the air-bloomery, before they were applied to the department of smelting.

Accident alone in almost every instance is the source or cause of invention. Anomalies in the arts and manufactures appear and vanish without notice or attention; and it is only when these fall under the observation of persons of investigating habits, that they are ever philosophically accounted for, or made subservient to useful or beneficial purposes. Bellows, in the first instance, were likely applied in the bloomery upon any occasional diminution of the usual or necessary current of air; by and by some advantage as to quality, produce, or time, might be procured, which would give rise to their more general introduction towards the concluding part of the process, in order more completely to separate the iron from its oxide and from the earths, and to unite the masses more firmly together, to withstand the shock of the hammer. The partial application of the bellows, however, was widely different from its more general application to the purposes of smelting. In the old process the pieces of ore were matured by a long period of cementation; but no perfect fusion or separation took place. The metallic particles coalesced, and part of the oxides and earths were discharged in consequence of their being fusible at a lower temperature; but the greater part of the expulsion, on which the purity and quality of the iron depended, took place under the hammer. By the introduction of smelting, a fusion comparatively perfect was obtained, and the charge of the furnace in a short time resolved into fluid scoria and metal. The basis of the operation, therefore, was completely changed; and it became the principal operation of the smith to remove the crudeness imparted by fusion to the iron, by such means and processes as occurred to him, or as experience pointed out, and to which has since been given the term of refining.

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[*] Communicated by the Author.

As this part of the operation would be the first and earliest attempt at the refining-furnace, so it is probable that the resulting bar-iron would be found in point of quality, toughness, and purity of fracture, superior to the iron obtained in the air-furnace. A conclusion would be formed by the smith in favour of the general use of bellows; and at this juncture those trials and experiments would commence, which terminated in the general introduction of the blast-bloomery, a furnace that for many ages maintained itself exclusively for the manufacture of iron over every iron-making country in Europe.

The furnace was constructed of stone capable of standing a high temperature, about two feet in height, and from a foot and a half to two feet square within, according to the power of the bellows: on the lower end of the furnace were two openings, one large in the front, the other of smaller dimensions behind, or on one side, for the insertion of the blast pipe. When the furnace was properly filled and heated with charcoal, a certain measure of ore and charcoal was added, according to the size of the bloom or blooms wished to be obtained, and which could be conveniently removed by the front opening in the furnace. The bellows were then urged. The charge melted, and the iron in the state of a crude steel, more or less separated from the ores, rested in an imperfectly fluid state upon the furnace bottom. Another operation therefore followed: After adding a measure of charcoal to cover effectually the surface of the metal, the nose of the bellows' pipe was inclined towards its surface, and part of the vitrid matter formed during the fusion allowed to run out. After blowing on the metal a certain length of time, it gradually by the burning out of the carbon became thick and lumpy. Iron bars were introduced at the front opening to break up the mass and to expose the lumps to the action of the blast, as is at present done in the charcoal refinery. When the operation was deemed complete, and the iron enough refined, the burning mass was removed through the front opening, either to the anvil or a large stone, and there beat into form.

In this improved state of the process, two distinct species of cinders, or scoria, were produced. First, in the smelting of the ores, which from the imperfect reduction contained about 20 per cent. of oxide of iron mixed with the earthy matter of the ore;—secondly, in the refining part of the operation, where the scoria produced contained from 70 to 80 per cent. of oxide of iron mixed with the earthy matter of the ores, and the vitrid waste of the interior of the furnace.

Abstractedly considered, the operation of the blast-bloomery furnace must be pronounced strictly philosophical, whereby was effected the double purpose of smelting and refining, in both of which large quantities of iron originally contained in the ore were lost. The operation itself, when well performed, was the mean between two extremes, which it must have been the interest and the anxiety of the smith to avoid. If the furnace was charged with too small a proportion of charcoal, or too large a relative quantity of iron ore, several evils would follow;—a lack of heat, a want of proper carbonation, and an imperfect fusion or separation; masses of ore would pass the blast unmelted, and mingle with the crude iron that had been reduced from the smaller masses of ore, presenting to the operation of refining an imperfect and unhomogeneous surface. On the contrary, an extra dose of charcoal, or a lessened relative proportion of ore, would lead to consequences the reverse of the former; under a higher temperature, an extra dose of carbon would unite with the iron, which would be precipitated on the bottom in a state too fluid for the usual length of time appropriated to the refinement. To divest the iron of its extra dose of carbon, the bellows would be directed upon its surface for a much longer time before it began to coalesce, or was tough enough to break up with the bars, a larger quantity of cinder would be discharged, and less of metal proportionate to the length of refinement.

When the operation was performed in the most perfect manner, not more than one-half of the iron was obtained from the ores. This gave rise to an immense accumulation of scoria, which in after ages, and subsequently to the invention of the blast-furnace, became a source of great wealth to the proprietor of land, who occasionally under ancient forests, or the deepest soils, discovered large quantities of these cinders, which were eagerly purchased by the smelters and used in the blast furnace, in mixture with ores, for the production of cast-iron.

Notwithstanding the small produce which in former times was obtained from the ores smelted in the bloomery furnace with charcoal as fuel, the late Colonel Fullarton attempted to

introduce

introduce the furnace, about five-and-twenty years ago, for the purpose of making bar-iron with coke and rich ironstones or ores. The blast-bloomery he employed was 3 feet in height, and from 2½ to 2¾ feet in width; like the old charcoal bloomery, it had an opening in front, and a blast-hole about nine inches from the bottom which admitted a pipe of 1⅛ inch in diameter. The furnace was filled with coke, gradually heated, and the blast introduced; then in small alternate portions were charged, four cwt. of ironstone, and in the whole 10 bushels of coke, containing about 700 pounds of fuel, or nearly the produce of one ton of raw pit-coal. In less than one hour the whole charge was smelted, the cinder was then tapped from the surface of the iron, upon which the blast-pipe was now inclined as in the old furnaces. After a period of six hours, the lump was deemed sufficiently malleablized; the front of the furnace was broken down, and the mass carried to the forge hammer. In this way about 150lbs of bar iron were obtained from 4 cwt. of Lancashire ore, or the same quantity of roasted ironstone of a very rich quality.

The Lancashire ore contained about 290lbs of iron; so of this quantity 130lbs only were recovered, or equal to 45 per cent. of the iron, and about 30 instead of 65 from the ore.

The ironstone was of a peculiar nature, a combination of iron and bituminous matter: so that, when roasted, the dissipation of the inflammable matter left a pure oxide of iron in thin beds or laminæ, which in the crucible yielded upwards of 70 per cent.; so that not 40 per cent. of the iron only was revived; and if the yield in iron is deducted from the ironstone in its raw state, two tons of which yielded about one ton of iron, the produce was not more than 20 per cent.

The whole operation, from filling to falling the furnace, took eight hours. The bloom in drawing out lost about eighteen pounds of iron; so that at the conclusion of the experiment not more than one hundred weight of finished bar-iron was obtained. A calculation founded upon this result, would give for one ton of bar-iron twenty tons of raw coal, and five tons of Lancashire ore; or, when used in preference, ten tons of ironstone, besides an immense increase of labour.

To accidental enlargement of the blast-bloomery, we most likely owe the discovery of cast-iron. With a view of improving the process, some enterprising smith may have added to the height of his furnace, and accidentally or by design enlarged the dose of charcoal. In tapping the furnace previous to refinement, he might be struck with a difference in the colour of the scoria, and the increased fluidity of the iron which might accompany it. These masses would be compared with former accidental productions, and the difference observed: at first they would most likely be returned into the bloomery to increase the general produce, but in time become the subject of a separate experiment. It would on trial be found, that they would melt in the common smithy, and by being blown upon in a manner similar to the refining part of the bloomery operation, pass into the state of malleable iron. This would first suggest the idea of a separate furnace and operation for the refinement of iron. The permanent enlargement of the bloomery furnace would ensue. The ores, by a greater descent and longer contact with the fuel, would be more thoroughly cemented. The iron would take up carbon in quantity, fusibility proportionally increase, and cast-iron in all its varieties, from the white conchoidal fracture to the large-grained deep grey, be obtained.

From this period, the making of iron would divide itself into two branches,—the furnace and forge departments; and, like other judicious divisions of labour, would give new spirit and enterprise to the art: in time foot-blasts and hand-blasts, or bellows, would be abandoned, and a more effectual moving power found on the banks of the adjacent streams. Water-wheels, giving motion to bellows and hammers, would succeed the crude and infantine efforts of a ruder age; and these in their turn would give way to other improvements, in a riper age, much more powerful and striking.

XLVII. *On the Application of Magnetism as a Measure of Electricity. By the Rev.* J. Cumming, *M.A. F.R.S. M.G.S. Professor of Chemistry in the University of Cambridge.*[*]

The methods hitherto in use for ascertaining the quantity and intensity of the electricity produced either by friction or by galvanic action, are (independently of the shock on the animal frame, which obviously affords no definite measure,) derived from its power in decomposing water, or fusing metallic wires. When the electricity is either small in quantity, or of low intensity, there are considerable difficulties in the practical application of either of these methods.

The fusion of platina wire by the elementary battery of Dr. Wollaston, proves that the quantity of electricity developed by very minute metallic surfaces is considerable; yet, exclusively of the difficulty in soldering wires that are barely visible, it is almost impossible to ascertain their length with any precision.

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[*] From the Cambridge Philosophical Transactions for 1822, Part II.

cision.—The other mode, that of measuring the quantity of water decomposed by a pair of small galvanic plates, is impracticable.—The recent discoveries of Professor Œrsted have enabled me to construct two instruments, one for discovering, the other for measuring, galvanic electricity, with a delicacy and precision that seem scarcely to admit of limitation.—The construction of the former instrument I have mentioned in a communication I had the honour to make to this Society some time since;—I shall now describe a few of the experiments I have as yet been enabled to make with it.—A wire of zinc and another of platina, each $\frac{1}{10}$ inch diameter, were coated with sealing-wax, so as to have merely their extremities exposed: on immersing them in a dilute acid, the circuit being at the same time completed through the galvanoscope, the needle deviated so decidedly, as to leave no doubt that a visible effect would have been produced by wires of less than half the dimensions of those I employed.—As the compass, though small, was by no means delicate, we may, I think, conclude from this experiment, that the electricity developed by two metallic surfaces, each $\frac{1}{300}$ of a square inch, may be detected, and their relations to each other ascertained, by this instrument.

The minute surfaces, and consequently small quantities of exciting fluid required for experiments with this instrument, offer the means of examining galvanic effects that have hitherto been unnoticed.

Of the acids whose galvanic effects I believe have not been examined, I have found, with small disks of zinc and copper, that the oxalic and hydriodic have considerable power; the phosphoric and acetic much less.—The action of strong sulphuric acid was inconsiderable, the needle being scarcely affected; but on adding a drop of water it deviated through more than half a right angle.—Were the galvanic action owing solely to the electricity developed by the metallic contact, the fluid acting merely as a conductor, according to a generally received hypothesis, the effect should be greatest when the stronger acid is used, concentrated sulphuric acid being a far better conductor than when diluted: on the other hand, since zinc or iron are readily oxidated by the action of dilute acid, though with difficulty when it is concentrated, this experiment seems to prove, that the galvanic action depends, not on the conducting, but on the oxidating power of the interposed fluid. —Hitherto I have not had the leisure to form that complete series of the electric relations of the metals towards each other, which this instrument affords the means of doing; yet the effects I have observed in two instances, are, I think, so remarkable, that I ought now to mention them.—On using two disks, one of iron, the other of steel, there was produced a decided deviation: since, then, the only difference in the metals arises from an alloy of from a $\frac{1}{60}$ to a $\frac{1}{140}$ part of the whole, it appears that this is sufficient to alter their electric relations.—The powerful affinity of potassium for oxygen, made it highly probable, that, in the galvanic circuit, it would become strongly negative with all the metals. On my first trial with disks of potassium and zinc, the potassium took fire before I could observe the effect; this difficulty I afterwards obviated by alloying it with mercury; on making the contact the needle deviated through nearly a right angle: The same effect was produced with copper; it was needless to try it with the other metals, for being negative with respect to zinc, and zinc being negative with respect to all the other metals, there can be no doubt that in the galvanic circuit, potassium is the most strongly negative metal with which we are acquainted. — It is perhaps scarcely necessary to remark, that, if any proof of the metallic nature of potassium were wanting, this experiment would have afforded it.

In using the magnetic needle as a measure of galvanic effects, we may either observe the deviation at a standard distance of the connecting wire from the needle, or assume a standard angle and measure the distance.—The latter method seems to have the advantage, as enabling us to use a smaller and therefore a more delicate needle, with this additional convenience, that the scale is increased in proportion as the length of the needle is diminished.—I therefore constructed an instrument, having a connecting wire fixed upon a moveable slide divided into inches and tenths, to which a vernier might be added if necessary. My first object was to ascertain the divisions on the scale, corresponding to variations in the angle of deviation;—for this purpose, the moveable wire was placed at different distances from the needle, increasing in arithmetical progression, and the corresponding deviations were observed. As the effects decrease very rapidly during the galvanic actions, the experiments were made as quickly as possible, proceeding from a distance of $\frac{1}{2}$ an inch to $10\frac{1}{2}$ inches, and again returning to the first distance. On taking the mean of several trials, made in this manner, I found that the tangent of the deviation varies inversely as the distance of the connecting wire from the magnetic needle.

It is well known that in a galvanic arrangement, intensity is given by the number, quantity by the magnitude of the plates; but I am not aware that any notice has been taken of the effects produced by varying their distances from each other. —On placing a moveable plate of zinc opposite a fixed copper-plate, I found, that, on diminishing the distance, the deviation of the needle placed under their connecting wire continued to increase, until they were in actual contact. The law of that increase, ascertained by the method I have just mentioned, was such, that the tangent of deviation varied inversely as the square root of the distances of the plates. In the construction of a Voltaic series composed of many plates, the advantages to be obtained by placing them very near each other, would be counterbalanced, by the risk of their intensity becoming sufficient to penetrate through a small distance; but in using large plates, with electricity of low intensity, it is obvious, that provided they are not in actual contact, they cannot be placed too near each other. By availing myself of this observation, I have been enabled to repeat, with a single pair of plates, the experiments of Ampère and Arago, which were originally performed with a battery of twelve pairs.—Of these, one of the most singular is that by which a spiral connecting wire is made to communicate permanent magnetism to a steel wire placed within in*.—This experiment I find may be varied, by using a straight connecting wire, and twisting round it a small steel bar; the zinc and copper ends of the bar receiving the northern or southern magnetism respectively, according as its spiral is from right to left, or the contrary. The repetition of this experiment led me to discover the cause of a singular effect, which I had the honour of exhibiting to this Society. The magnet, which was deprived of its attractive power when its north pole was connected with the zinc wire of a pair of galvanic plates, had been placed in the circuit, by twisting the wire round its poles from left to right: on making this spiral from right to left I reversed the effect; and when the spirals round its two poles were in opposite directions, the weights suspended from them were oppositely affected at the same time, the attractive power of one pole being increased when that of the other was destroyed.

The singular effects produced by using a large conducting wire, I have mentioned in my former paper on this subject, and the analogy it forms between the galvanic and the common form of magnetism.—In the further examination of this diffusion of the magnetic influence, I have found it to be far more extensive than I had at first imagined.—On making the connexion between a pair of plates containing about 1½ foot of surface, through a copper globe of more than a foot diameter, and therefore containing full four square feet of surface, every part of it exhibited magnetic effects, either upon a horizontal or a vertical needle.—The same effects were manifested whatever were the forms of the surfaces interposed between the galvanic plates.—On varying the experiment by connecting both extremities of the plates with each other, by means of small wires, so that there was a metallic circuit throughout, (in which case it is generally conceived that all galvanic effect ceases), I found that every part of this circuit affected the magnetic needle.

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* In these, as in the previous experiments, I find that if the spiral be made of large wire it is much more efficacious than if of small.

The magnetism of the connecting wires was examined in the usual mode; that of the plates themselves by immersing in the exciting fluid a small compass in a glass case, made impervious to the water. It is perhaps premature to form any theoretical opinions upon these few facts, which seem to me adverse to the received opinion, that the galvanic effects are produced by the decomposition of an electric fluid circulating between the positive and negative plates; yet, if ever the mysterious agency of galvanism is to be detected, it must be by examining it in its simplest form; and this, the discovery of the connexion between galvanism and magnetism, and the delicacy of the instruments it enables us to apply, seems to promise, more readily than any modes yet tried, the means of accomplishing.

Since this paper was read to the Society, I have had an opportunity, with the assistance of Dr. Clarke and Mr. Lunn, of trying the magnetic effects of atmospherical electricity.—A wire of about 100 yards in length, connected with a kite, readily magnetised a steel needle inclosed in a spiral wire, but caused no deviation in a compass placed beneath it.—I have obtained the same results in repeating Sir H. Davy's experiments both with the Leyden jar and with sparks taken from the conductor of an electrical machine.—It seems that the galvanic magnetism is most readily made sensible by the deviation it causes in the compass needle; but the electrical by its power of communicating permanent magnetism.

The experiment on atmospherical electricity suggests an easy method of ascertaining, from time to time, the prevalent electricity of the air; by inclosing small steel bars in a spiral wire connected with a conducting rod, and examining the magnetism induced in them.

XLVIII. *Report of a Committee of the House of Commons on Steam-Boats.*

[Concluded from p. 132.]

A List of all the Steam-Boats built since 1811: showing their Tonnage, and the Power of their Engines; the Names of the Builders and of the Engine-Makers; the Dates of their being launched, and also the Stations where they ply. [From the Appendix referred to, *supra*, p. 119.]

When built.	Names of Vessels.	Tonnage.	Nominal Power. Total Horse Power;	or,	Horses Power.	Where employed.	Where or by whom built	Names of Engineers.
1812	Comet	25	4			Glasgow and Helensburgh Baths	J. Wood & Co. Port Glasgow	Mr. Bell.
—	Elizabeth	40	9			First on the Clyde, now on the Mersey ...	.... Ditto	Mr. Thomson.
1813	Clyde	69	14			Glasgow and Greenock	... Ditto	Mr. Robertson.
—	Margery	70	14			On the Clyde; and in 1815 sailed from Leith to London; and in 1816 to the Seine	....	Ditto.
—	Glasgow, now the Thames	74	16			Glasgow and Largs; in 1815 sailed round the Land's End to London, and plied between Margate and London	J. Wood & Co.	Mr. Cook.
—	... ...	28	4			Bristol and Bath	Bristol.	——
—	Orwell	60	12	2	6	Ipswich and Harwich	Lepingwell, Yarmouth	Aggs & Kerr, Norwich.
—	... ...	20	3	Horizontal. High press.		Norwich and Yarmouth	A captured French Row Boat	Wright, Yarmouth.
—	Prince of Orange ...	40	8	2	4	Glasgow and Gourock	Clyde	Boulton & Watt.
1814	Stirling Castle	60	12			Inverness and Fort Augustus ..	....	Mr. Bell.
—	Lady of the Lake ...	76	20			Leith and Stirling; 1816 crossed to the Elbe, and plied between Hamburgh and Cuxhaven	Gray, Kincardine	Cook, Glasgow.
—	Morning Star	100	26			Leith and Alloa.	——	——
—	... ...	20	3	High press.		Norwich and Yarmouth	Wright, Yarmouth ..	Wright, Yarmouth.
—	Phœnix	25	4	High press.		Ditto (this engine exploded in 1816)	... .	Watts, Yarmouth.
—	Eagle (double vessel) ...	40	6			Went up the Seine in 1815	....	Wright.
—	Richmond	60	10			London and Richmond	Lepingwell, Yarmouth	Maudslay & Co.
—	Perseverance	60	14			Newcastle and Shields	Boult, South Shore ..	Crowther, Newcastle.
1815	Caledonia	102	32	2	16	First on the Clyde; and in 1816 came round the Land's End to London; 1817, was refitted by Boulton and Watt; went up the Rhine, and in 1818 to Copenhagen.	Wood & Co.	Greenhead & Co.
			28	2	14		....	Boulton & Watt.
—	Argyle	88	32	2	16	Glasgow and Inverary	Wood & Co.	Greenhead & Co.
—	Oscar	70	12			Glasgow and Greenock	Ditto	Robertson.
—	Swift	12	3			Newcastle and Shields	——	——
—	Hope	45	6	2	3	Chatham and Sheerness	Bristol	Johnston.

1816	Regent	112	24	2	12	London and Margate; destroyed by fire in 1817	Courthope, Rotherhithe	Maudslay & Co.
—	Majestic	90	24			London & Margate; now London & South-end	Court, Ramsgate	Dixon, London.
—	Congo	100	20			Intended for the Congo expedition	King's Yard, Deptford	Boulton & Watt.
—	Caledonia	80	12			Selby and Hull	Smart, Dundee	Robertson.
—	Neptune	88	40	2	20	Glasgow and Inverary	Wood & Co.	Cook.
—	Sir William Wallace	95	32	2	16	Newhaven and Kinghorn	Ditto	Ditto.
—	Albion	92	22			Largs and Glasgow	Ditto	M'Arthur.
—	Ætna (double vessel)	75	20			Across the Mersey at Liverpool	Liverpool	Fawcet & Littledale.
—	Eagle	70	20			Newcastle and Shields	Deeg, Newcastle	Boulton & Watt.
1817	Tug	95	32	2	16	Leith and Stirling, and towing	Wood & Co.	M'Arthur.
—	Defiance	50	14			Glasgow and Loch Gilphead	Ditto	Robertson.
—	Marion	70	14			Loch Lomond	....	Napier.
—	John Bull	75	15			Gainsborough and Hull	Moody, of Thorn	Butterly Company.
—	Humber	80	12			Ditto	Brown, Perth	Robertson.
—	Britannia	70	15			London and Southend	Titterton, of Stockwith	Butterly Company.
—	London	70	14			London and Richmond	Searle, Westminster Bridge	Ditto.
—	Sons of Commerce	80	20			London and Gravesend	Lafort, Blackfriars	Ditto.
—	Enterprize	30	5			Newcastle and Shields	Cockburn, St. Peter's Quay	Robson.
1818	Rob Roy	100	30			Glasgow and Belfast; now Dover and Calais	Denny, Dumbarton	Napier.
—	Lady of the Shannon	90	20			.. ..	Scot & Sons, Greenock	Cook.
—	Marquis of Bute	60	14			Glasgow and Gourock	Wood & Co.	Robertson.
—	Engineer	315	70	2	35	London and Margate	D. Brent, Rotherhithe	Maudslay & Co.
—	Rising Star	400	70	2	35	Gone to South America	Ditto	Ditto.
—	Favourite	160	40	2	20	London and Margate	Lafort, Blackfriars	Boulton & Watt.
—	Victory	160	40	2	20	Ditto	Evans, Rotherhithe	Dixon.
—	British Queen	75	20			Gainsborough and Hull	Smith, Gainsborough	Brunton, Birmingham.
—	Albion	75	24			Ditto	Ditto	Horsly Company.
—	Cobourg	75	24			Southampton and Cowes	Ditto	Ditto.
—	Selby	80	24			Hull and Barton	Foster, Selby	Overton & Smith.
—	Speedwell	40	10			Newcastle and Shields	Dixon, South Shore	Robson.
1819	Robert Bruce	155	60	2	30	Liverpool, Isle of Man, and Greenock; burnt in 1821	Scot & Sons, Greenock	Napier.
—	Talbot	156	60	2	30	Holyhead and Dublin; now London and Calais	Wood & Co. Port Glasgow	Ditto.
—	Waterloo *	210	60	2	30	Liverpool and Dublin	Scot & Sons	Cook.
—	Robert Burns	73	24			Glasgow and Helensburgh	Wood & Co.	Napier.
—	Port Glasgow	70	16			Ditto	Ditto	Murdock.

(continued)

When built.	Names of Vessels.	Tonnage.	Nominal Power.			Where employed.	Where or by whom built.	Names of Engineers.
			Total Horse Power;	or,	Horses Power.			
1819	Mersey	80	24			Across the Mersey at Liverpool	....	Fawcet & Littledale.
—	... ...	60	20			Liverpool and Runcorn	....	Brunton, Birmingham.
—	Eclipse	190	60	2	30	London and Margate	D. Brent, Rotherhithe	Boulton & Watt
—	Maria Tug	80	24			Hull and Gainsborough	Smith, Gainsborough	Horsly Company.
—	Favourite	117	26			Hull and Selby	Ditto	Ditto.
—	Countess of Scarborough	50	10			Gainsborough and York	....	Overton & Smith.
—	Hope	30	6			Newcastle and Shields	Dixon, South Shore	Robson.
—	Swift	9	3			Ditto	Emerson, South Shields	Ditto
1820	Superb	246	70	2	35	Liverpool, Isle of Man, and Greenock	Scot & Co.	Napier.
—	Ivanhoe	158	56	2	28	Holyhead and Dublin	Ditto	Ditto.
—	Inverary Castle	114	40	2	20	Glasgow and Inverary	Wood & Co.	Ditto
—	Belfast	190	70	2	35	Liverpool and Dublin	Ritchie & Co. Belfast	Ditto.
—	Britannia	100	40	2	20	Glasgow and Campbeltown	Wood & Co.	Cook.
—	Rothesay Castle	90	24			Glasgow and Isle of Bute	....	M'Arthur.
—	Glasgow	90	24			Glasgow and Coast of Ayr	——	——
—	Earl of Egremont	50	24	2	12	Towing for Chichester and Arundel Canal Company	Canal Company, Milton, near Portsmouth	Maudslay & Co.
—	Diana	60	20	2	10	London and Richmond	Evans, Rotherhithe ..	Boulton & Watt.
—	Aire and Calder	110	35			Hull and Selby	At Selby; Patent Cross Planking	Baker, Putsor, near Leeds.
—	Leeds	125	30			Ditto	Gutteridge, Selby	Horsly Company.
—	Caledonia	80	30	2	15	Dundee and Perth	Smart, Dundee	Carmichael, Dundee.
—	Tyne	40	10			Newcastle and Shields	Bolt, Newcastle	Robson.
—	Two Brothers	35	9			Ditto	Davidson, South Shields	Gibson.
—	Indefatigable	30	8			Ditto	Hutson, Howden Pans	Hawthorn.
—	Duchess of Northumberland	40	10			Ditto	Wright ditto	Ditto
1821	Majestic	350	100	2	50	Liverpool, Isle of Man, and Greenock	Scot & Sons, Greenock	Napier.
—	Mountaineer	190	70	2	35	London and Leith, now Liverpool and Dublin	Cornwallis, Greenock	Ditto.
—	Eclipse	140	60	2	30	Glasgow and Belfast	Steel, Greenock	Ditto.
—	Rapid	140	56	2	28	Ditto	Cornwallis	M'Arthur.

1821	Post Boy	80	20			Glasgow, Dumbarton and Greenock	....	Napier.
—	Highlander	67	24			Glasgow, Fort William and Western Islands	Wood & Co.	M'Arthur.
—	Comet	70	30			Ditto	——	——
—	Tartar	180	60	Horiz. engine.		Holyhead and Dublin	Government vessel altered under Mr. Brodrip's direct.	Cook.
—	Caledonia	84	30			Glasgow and Helensburgh	Wood & Co.	Napier.
—	Brilliant	160	40	2	20	Leith and Aberdeen	——	——
—	Velocity	150	40	2	20	Ditto	——	——
—	Sampson	100	40	2	20	Towing for Glasgow and Liverpool Shipping Company	....	M'Arthur.
—	Edinburgh Castle	148	40	2	20	Newhaven and Kinghorn	Wood & Co.	Cook.
—	Thane of Fife	148	40	2	20	Ditto	Ditto	Ditto.
—	Star	90				Ditto	——	——
—	Queen Margaret	100				Ditto	——	——
—	Surprise	120				Stirling, Grangemouth and Leith	——	——
—	Tourist	200	90	2	40	London and Leith	Brown, Perth	Gutzmer, Leith Walk.
—	Union (double Ferry Boat)	100	30	2	15	Dundee Ferry across to Fife	Ditto	Carmichael, Dundee.
—	Swift	250	80	2	40	Brighton and Dieppe	Leith Smack lengthened	Gutzmer.
—	Royal Sovereign, George IV*	205	80	2	40	Holyhead and Dublin Post-Office Packet	Evans, Rotherhithe	Boulton & Watt.
—	Meteor *	190	60	2	30	Ditto	Ditto	Ditto.
—	Dasher *	130	40	2	20	Dover and Calais Post-Office Packet	Ditto	Ditto.
—	Arrow *	130	40	2	20	Ditto	Patterson, Rotherhithe	Ditto.
—	City of Edinburgh	400	80	2	40	Leith and London	Wigram, Blackwall	Ditto.
—	James Watt	448	100	2	50	Ditto	Wood & Co. Port Glasgow	Ditto.
—	Venus	265	60	2	30	London and Margate	Evans	Ditto.
—	Eagle	170	40	2	20	London and Ramsgate	Brocklebank, Deptford	Ditto.
—	Swiftsure	104	30	2	15	London and Gravesend	Wigram, Blackwall	Ditto.
—	Hero	233	90	2	45	London and Margate	Bancham, Finsbury on the Medway	Murray & Fenton, Leeds.
—	Cambria	130	50	2	25	Liverpool and Bagilt	Mottershead & Hayes	Fawcet & Littledale.
—	Britannia	80	20			Across the Mersey	Ditto	Dove & Co. Liverpool.
—	Abbey					Ditto	C. Grayson	Ditto.
—	Portuguese	80	20	2	10	Ditto	Humble & Harvey	Fawcet & Littledale.
—	Lady Stanley	87	20			Ditto	Mottershead & Hayes	Dove & Co.

* These four vessels were built under the direction of Mr. Lang of the Navy Office.

When built.	Names of Vessels.	Tonnage.	Nominal Power. Total Horse Power;	or,	Horses Power.	Where employed.	Where or by whom built.	Names of Engineers.
1821	Navigator	40	18			Newcastle and Shields	Hutson, Howden Pans	Hawthorn.
—	Safety	96	14			Ditto	Davidson, Newcastle ..	Hawkes.
—	Union	20	4			Ditto	Bennet, South Shields ..	Gibson.
1822	City of Glasgow	300	100	2	50	Liverpool, Isle of Man, Portpatrick, & Glasgow	Scot & Sons, Greenock	Napier.
—	Rapid	140	60	2	30	London and Rotterdam	Cornwallis, Greenock ..	Ditto.
—	St. Patrick	298	110	2	55	Liverpool, Dublin, Tenby and Bristol ..	Mottershead & Hayes ..	Fawcet & Littiedale.
—	St. George	312	110	2	55	Liverpool, Isle of Man, Portpatrick, & Greenock	Dawson & Pearson ..	Ditto.
—	Duke of Lancaster ..	141	50	2	25	Liverpool, Whitehaven, Isle of Man, and Dumfries	Mottershead & Hayes	Ditto.
—	Hercules	130	60	2	30	Towing in the Clyde	....	M'Arthur.
—	Towart	120	50	2	25	Glasgow and Inveraiy	....	Ditto.
—	Highland Chief	65	16			Stranraer	——	——
—	Albion	160	60	2	30	Liverpool and Bangor, along the Welsh Coast	Mottershead and Hayes	Fawcet & Littledale.
—	Prince Llewellyn ..	170	70	2	35	Ditto	Wilson & Co.	Ditto.
—	Bristol Cambria	100	30			Bristol	J. James	Dove & Co.
—	Lord Melville	220	80	2	40	London and Calais	Jolliffe, Banks & Co.	Butterly Company.
—	Sir Joseph Yorke ..	100	30			London, Southend and Sheerness	Ditto	Ditto.
—	Royal Sovereign ..	220	80	2	40	London and Ramsgate	Brocklebank, Deptford	Ditto.
—	Sovereign	95	32	2	16	Dover and Boulogne	James Duke, Dover ..	Maudslay, London.
—	.. ..	95	32	2	16	Ditto	Ditto	Ditto.
—	Union	53	16	2	8	Ditto	Evans	Yates.
—	Medusa	90	20	2	10	Ditto	Ditto	Horsly Company.
—	Medina	100	36			Southampton to Cowes	——— Cowes	Ditto.
—	Aaron Manby	140	28			On the Seine	Iron Vessel; built by Mr. Manby, Horsly Works, Tipton in Staffordshire	Ditto.
—	Yorkshireman	200	80	2	40	London and Hull	Pearson, Thorn	Murray & Fenton, Leeds.
—	King of the Netherlands	140	40	2	20	London and Rotterdam	Wigram	Boulton & Watt.
—	Lemington Packet ..	30	7			Tyne	Bolt	Hawthorn.

XLIX. *On the Repeating Circle improved according to the Suggestion of Baron* Zach: *and on taking Observations of the Pole-Star. By Professor* Littrow, *of the Imperial Observatory of Vienna**.

I have not yet spoken to you of my repeating circle, of eighteen inches, by Reichenbach and Ertel, constructed here at Vienna; but it is because I received it only at the beginning of August (1820): I have however made a good number of observations with it of the latitude.

This circle is furnished with two excellent levels with air-bubbles; one of them being, according to your recommendation, made fast to the outer circle, or circle of divisions, in order to be certain of its invariable position during the conjugate observation. I have found the addition of this level very necessary; for in fact, when the vernier or inner circle which carries the telescope is turned, small oscillations are always remarked in the bubble of the level, although strongly connected with the circle of divisions by the clamp and its screw. The bubble does not return to its place until the vernier circle and its telescope are returned to their old position;—an evident proof that the circle of divisions does not continue fixed, and that it does not always return to its old position after the movement of the vernier-circle, which had hitherto been always tacitly and falsely supposed. You were the first to point out this defect, and long since cautioned astronomers and artists respecting it: at length it has engaged their attention. I have always attended to this fixed level of the circle of divisions, which indeed renders the observations a little more troublesome, but in return much more certain and accurate.

In order to fix the latitude of my observatory, which has appeared to me to be by no means well determined, I have made choice of the pole-star for reasons known to all astronomers. Though the telescope of my circle is but 20 inches in length, it is such an excellent one that I can see the pole-star with it, without the smallest difficulty, at any hour of the day, and even at bright noon. Impatient to collect in a little time a great number of results, I did not content myself with two meridian transits a day, but had recourse to a method which I first proposed in the third volume, page 208, of Messrs. Lindenau and Bohnenberger's Astronomical Journal, and which consists in taking the altitude of the pole-star at any time whatever. I have since noticed with pleasure that several astronomers have practised this method, which on account of the great liberty it gives the observer, to take his observations at any time, not subjecting him to a time determined and limited, deserves to be generally received, particularly by travelling astronomers and mariners.

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* Translated from a Letter, dated Vienna, November 23, 1820, to Baron Zach, published in his *Correspondance Astronomique*, &c.

At all times observers have contented themselves with taking meridian altitudes of the pole-star at its two transits in the four-and-twenty hours. These two points, doubtless, are the most advantageous for obtaining the latitude of the place of observation, independently of the declination of the star. Latterly, it has been proposed to take the altitudes of this star at the instants of its utmost digressions toward the east and west. These points are much less favourable, especially when the time is not determined with the greatest strictness; on the contrary, it appears to me, that any other point on the parallel of this star is preferable to those two points, as I shall have the honour of showing you.

Let z be the zenith distance of the star; p its apparent distance from the pole of the equator; t the horary angle, $90-\psi$ the latitude sought; we shall have, by supposing the distance p small, as it is for the pole-star,

$$d\psi = \frac{\sin.\ z}{\sin \psi \cos.\ p}\, dz - \text{tang.}\ p \sin.\ t.\ dt + \cos.\ t.\ dp.$$

Hence it appears that an error in the observed zenith distance produces, every where, nearly the same error in the latitude; and this is likewise the case for the meridian transits, which in this respect are not preferable to all the other points of the parallel of this star. As to the error in declination, it is very little to be feared in the pole-star so well determined: besides, the error that would result for the latitude is less in all the other points of the parallel than in those of the meridian transits, which under this point of view would be the least advantageous. Again, let us consider the error of the time. It is true that this error does not influence the observations made in the meridian, and in this case they seem preferable to the others. But when it is considered that the factor dt in the preceding formula is *tang. p. sin. t*, it is seen that an error in the time has a very small influence on the latitude, as well as in every other point of the parallel. Let us suppose the error in time to be one second or fifteen seconds in arc, we shall have for the horary angles of 6 hours...4 hours...2 hours,
errors in latitude of 0″·4 0″·3 0″·2.

All astronomical observers will agree that an error of 0″·4 in arc, is inappreciable, that it is impossible to answer for it with our largest circles, and our most perfect instruments. In every case, we shall still have the means of eliminating that error, having only to take altitudes at an equal distance on the other side of the meridian, where the error $d\psi$ changes its sign and is destroyed. It is obvious, from all these considerations, that for the accuracy of the result it is indifferent to observe the pole-star when it passes the meridian, or to observe it in any point whatever of its parallel; but for the convenience of the observer, and for the infinitely valuable advantage of being able to collect a great many good observations of latitude in a short time, my method seems to deserve the preference before all others. The observer does not depend on a single instant which occurs in 12 or 24 hours, and which bad weather, a cloud, or other accidents, may render unavailable. With my method he may take the star's altitude at any time of the day or night, at his pleasure, the atmosphere permitting, or when he shall have the leisure and the inclination. In 24 hours he may take as many observations as he pleases, and collect in that time a great number of latitudes. I have therefore employed this very method to determine the latitude of my observatory, and you will judge, sir, whether the observations which I have the honour to send to you deserve any confidence. I shall add a few words more on the method of calculating them.

I have made a little table, which with the argument t furnishes me with the two quantities m and n given by the following expressions:

$$m = \frac{\sin.\, p\ \sin\, \psi}{\sin.\, z} . \sin.\, t.$$

$$n = m \text{ cotang. } t.$$

Let θ, θ', θ'' ...... be the differences of the times of observation, and of the middle of all those times, I seek in the well-known table in the hands of all astronomers the quantity

$$\text{A} = \frac{2 \sin.^2 \frac{1}{2} \theta}{\sin.\, 1''} + \frac{2 \sin.^2 \frac{1}{2} \theta'}{\sin.\, 1''} + \frac{2 \sin.^2 \frac{1}{2} \theta''}{\sin.\, 1''} + \&c. \ldots\ldots$$

Having obtained by these two tables, almost without calculation, the quantities m, n and A, the rest of the operation is very easy. Denoting by N the number of repetitions, we have

$$dz = (n - m^2 \text{ cotang. } z)\ \frac{\text{A}}{\text{N}},$$

$$\left.\begin{aligned} \text{Tang. } x &= \text{tang. } p.\ \cos.\, t. \\ \text{Cos. } (\psi - x) &= \frac{\cos.\, x}{\cos.\, p.}\ \cos.\, (z - dx) \end{aligned}\right\} (1)$$

This calculation may be further simplified by constructing a little table to give the value of the quantity $n - m^2$ cotang. z, which depends on the argument t, and then the first table will be useless. Every calculator at all practised in this kind of calculation, will still find other methods for abridging the little calculation of the quantities x and ψ: all these little artifices, as well as the demonstrations of these expressions, are too easily found to require my dwelling upon them longer.

When the altitudes of the pole-star are taken with a reflecting sextant, or with a non-repeating instrument, and even with a repeating circle if the observations are not carried beyond from four to six repetitions, the correction dz, which is very small, may be neglected, keeping merely to the last two equations (1).

L. *Analysis of Air taken from an Ice-House.* By John Murray, *F.L.S. M.G.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

9th July, 1822.

Gentlemen,—Mr. Parker of Sweeney Hall, near Oswestry, having erected an ice-house on a construction which seems well adapted for the preservation of ice, mentioned to me that the included air was *incapable of supporting combustion;* and that the person employed felt great difficulty in breathing, and requested me to analyse it. As it presents some curious phenomena, perhaps you will do me the favour to give insertion to an account of it.

On the 24th of May last, at 11 A.M., the temperature of the atmosphere under the thatched roof above the ice-house was 61° 5′ Fahrenheit; while in the ice-house itself the thermometer indicated 63° 5′ Fahrenheit, almost in contact with the ice!!

A candle let down, no sooner entered the dome than it was extinguished. A bucket was then lowered down, and being drawn up again, it was found to contain an atmosphere which immediately extinguished a candle.

Two phials with glass stoppers, accurately fitted and filled with water, were emptied in the ice-house; and the stoppers further secured by slips of bladder tied over them: their contents were subjected to a careful analysis. A glass tube containing a cubic inch, and graduated into 100 parts, was filled with this air over water: a chip of pure caustic potassa being introduced and agitated therein absorbed 5 parts. The residual air being afterwards brought in contact with nitrous gas, 16 parts more were absorbed by water. In a subsequent experiment 32 parts of hydrogen were added, and exploded by the electric spark. The remaining air, after being agitated in contact with pure potassa, was found to be unmixed azote; so that 100 parts were composed of

05 carbonic acid gas,
79 azote,
16 oxygen:

consequently the carbonic acid gas seemed to be formed at the expense of the oxygen of the atmosphere; 5 parts in 100 being supplanted by an equal volume of carbonic acid gas. The decomposition of the straw, from contact with the melted ice, appears to be the cause; and the increased temperature may be considered as the result of this extraordinary fermentation: certainly extraordinary if the temperature at which ice is maintained be considered. The masses of ice which I have seen taken from this ice-house during June last, seemed to be well preserved.

I examined, by chemical re-agents, the ice taken from this ice-house, by melting the ice previously washed with distilled water, and also by dissolving it in distilled water. Oxalate of ammonia scarcely affected the fluid in a notable degree: the effect was equivocal. Nitrate of silver seemed to form very delicate silky strings; and with lime-water, or caustic baryta, &c., it yielded no trace of carbonic acid gas. The carbonic acid gas, therefore, could not proceed from the ice.

The ice in question was procured from an adjoining pond, and thrown into the ice-house in January last. In about two months, it had shrunk from the wall eight inches, and this vacuity was then filled up with straw, and a covering of the same thrown over it.

A lantern let down into an adjoining well of considerable depth continued to burn with undiminished flame. The ice-house is in a dry sandy bank, environed with trees.

I have the honour to be, gentlemen,
Yours most obediently,
J. MURRAY.

LI. *A Defence of the new Theory of the Tides.* *By Captain* FORMAN.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—NOTWITHSTANDING there is a little asperity in the style of Mr. Henry Russell's Reply to my New Theory of the Tides, I am really obliged to him for the notice he has taken of it; because I am fully persuaded that the more it is discussed, the more the public will be convinced of the utter impossibility of accounting for the rising of the tides, without supposing expansion in the particles of water to be the immediate cause of the phænomenon.

Mr. Russell agrees with me that the waters do press downwards towards the centre at the time the tides are rising, and he admits that they are not pulled upwards by the power of the moon's attraction; but, instead of allowing that they are pushed upwards by the expansion of their own particles, he supposes that they are pushed upwards by the downward pressure (*some thousands of miles off*) of the waters where the tide is ebbing, or, to use his own words, "by the superior gravity of those waters that constitute the ebb."

Now I recommend to Mr. Russell to fill a box with marbles, or any other hard round substances, and then try whether an additional weight at one end will cause the other end to rise up. Mr. Russell, I suppose, will not maintain that there is an immense syphon at the bottom of the ocean: and, as he has not thought proper to explain it himself, I can imagine no other way by which he can make good his hypothesis. Whether he can, or whether he cannot, is however of very little moment, because I can prove, in hundreds of instances, that the tides are rising when his supposed cause has no existence; and if I can make this evident, as Mr. Russell has gone so far as to acknowledge that the waters are not pulled upwards by the power of the moon's attraction, he must either admit my theory to be true, or be guilty of the absurdity of maintaining that an effect may be produced without a cause. If Mr. Russell will allow that the moon's attraction is the primary cause of the rising of the tides, he must admit that in the open ocean the tides are rising at the same time all the way as far as 90° to the westward of any place where it is just high water. Now the Atlantic Ocean is not more than 60° or 70° broad, and consequently, when the tides are still rising a little before high water, in the Bay of Biscay, they must be also rising over the whole breadth of the Atlantic Ocean. It is evident from this, that the tides may be rising on one side of the Atlantic when there is no ebb on the other; and therefore, unless we are to suppose that the waters are rising upwards and pressing downwards at the same time, (which would be to acknowledge my principle of expansion, and to do away with the necessity of Mr. Russell's hypothesis,) the rising of the tides cannot be produced by such a cause.

Mr. Russell further argues, that "the circumstance of the highest tides being invariably accompanied by the lowest ebb, is alone sufficient to convince any impartial inquirer that the ebb and flow of the waters are produced by changes of place, and not by rarefaction and condensation alone."

This objection however is very easily answered. Mr. Russell of course is aware that the Newtonian philosophers account for the ebbing of the tides by supposing that the moon, when she is below the horizon, pulls the waters (*obliquely*) downwards by the power of her attraction; and it must be very evident that the nearer the moon comes to the earth the greater will be the power of her attraction; and also, as the direction or inclination of that power will be less oblique, the effect produced by it will be so much the more. Now it is acknowledged by all the astronomers, that the moon is nearer the earth on the full and change days than when she is in her quarters; and consequently her power to press the waters downwards must be so much the greater. Mr. Russell must also be aware, that at the time of the spring tides the sun is always either in conjunction or opposition with the moon: but at the neap tides the power of the sun's attraction is always opposed to that of the moon, and is always pulling the waters upwards in those places where the moon's attraction is pressing them down, that is, where it is low water by the moon's tide; so that the waters, at low water, during the neap tides, must be so much higher than they are at spring tides, as the sun's attraction has power to lift them up.

"The satisfaction" (says Mr. Russell) "which Mr. Forman seems to derive from a handful of water is by no means enviable. He speaks of the moon as if astronomers and philosophers think its attractive power over substances on the face of the earth equal to the attractive power of the earth itself: but I believe there are very few philosophers that are not satisfied of the contrary. Surely he does not wish us to understand that because the moon has no power to sustain a handful of water* in the atmosphere, it has no power over it whatever. He may as well attempt to teach us, that because a magnet has no power to lift a scale beam, it has no power to disturb its equilibrium."

If Mr. Russell had taken the pains to inform himself of the cause of the equilibrium of a suspended weight (of iron) being disturbed by a magnet when the same magnet has not sufficient power to lift it up, he would never have ventured to make use of it as an argument against my theory. If we suppose a body to be suspended by a cord, it is evident that the whole weight of this body must be sustained by the cord; but if this body should be pushed away from the perpendicular by any person, then the weight will be divided between this person and the cord in proportion to the angle it makes with the perpendicular. Thus the touch of a child may be able to disturb the equilibrium of a ton weight when it is suspended; but it would require a force that is able to lift five hundred weight to keep it at an angle of 45° from the perpendicular; and upon the same principle, a magnet may be able to disturb the equilibrium of a suspended weight of iron, though it has no power to lift it up. If Mr. Russell meant to found any argument upon this fact, he should first of all have shown that the particles of water are not supported from beneath, but are suspended by some invisible power above; for otherwise there can be no analogy in the two cases.

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* And consequently has no power to lift a handful of water.

Mr. Russell asks, Whether I mean to be understood that, because the moon has no power to lift a handful of water, it has no power over it whatever? I answer, that the effect of the moon's attraction is to take off a portion of the gravity or weight of every particle of water, and thereby produce an expansion upwards of every one of these particles, and consequently a rise of the waters equal to the sum of the expansion of the whole. As Mr. Russell has not made good his hypothesis, the question still remains as it was, and I again challenge those philosophers who deny the fact, to account for the rising of the tides, without supposing that the waters are lifted up by the expansion of their own particles.

Here I must take the liberty of hinting to Mr. Russell, that nothing betrays the weakness of a cause so much as the putting a meaning into the mouth of an adversary which never was intended. Mr. Russell could not possibly have imagined, that I supposed it was the opinion of philosophers that the power of the moon's attraction was equal to the earth's; because all my arguments are founded on the direct contrary supposition. So far, in fact, was I from supposing any thing of the kind, that I was even apprehensive that they would not allow me my own position, that the power of the moon's attraction was equal to the 2400th part of the earth's; for, if I could have hoped they would have admitted it, I would willingly have estimated the power of the moon's attraction at the 500th* part of the earth's, (which I believe is much nearer the truth,) and then I should not have required a greater depth of ocean than five-and-twenty or thirty miles.

How far the satisfaction I derive from the falling of a drop of water may be enviable, is what I am very indifferent about; but the argument possesses all the force I wished it to have. If the moon's attraction has no power to prevent a drop of water from falling, it certainly can have no power to lift it up; and consequently the waters must either be raised by the expansion of their own particles, or they must be pushed upwards in the manner Mr. Russell has supposed; and I leave it to the philosophers to make their choice between the two hypotheses.

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* In the course of the month I intend to publish a very small pamphlet on this subject, which will clear up a great many difficulties that were not satisfactorily explained in my former theory.

I am, gentlemen,
Your most obedient servant,
Bath, August 2, 1822. WALTER FORMAN.

P.S.—I cannot take leave of this subject without pointing out to the notice of your readers a most extraordinary feature in Mr. Russell's mode of *arguing* (if I may be allowed that expression), which is certainly unique in the annals of philosophy. Mr. Russell denies my hypothesis, and proposes another; but he does not think it necessary either to prove that his own is true, or that mine is false. He *asserts* that the tides are raised "by the superior gravity of those waters that constitute the ebb;" but where are his proofs? He does not explain in what way the downward pressure of the waters on one part of the globe can cause the waters on another part to rise up; and yet he expects me to combat a position, when it is morally impossible that I can understand what he means.

As however he has thought proper to throw the burden upon my shoulders, I am ready to undertake it; because, though I cannot be expected to combat an argument before it is put, it is easy to show that his hypothesis cannot possibly be true. Mr. Russell, I suppose, is so much of a philosopher as to know that, whenever it is low water in any place (let him choose where he will), the tides are rising on one side of it and ebbing on the other; and, if the "superior gravity" of the water in this place cannot prevent the waters from ebbing on one side, it is not very philosophical to suppose that it can lift the waters on the other.

LII. *On reducing the Lunar Distance. By* HENRY MEIKLE, *Esq.*[*]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—IN my communication which you had the goodness to insert in the Philosophical Magazine for September last, a method was proposed for constructing a general plate for reducing the lunar distance; and its leading feature, so far as relates to parallax, was shown to consist in the arrangement of three parallel straight lines in such a manner that when two of them served for the altitudes, the effect of parallax on the distance might by help of a ruler be read off from the third. The same general principle, it was also remarked, admitted of several forms of construction; and since that time I have made some comparison among these different forms, with the view of ascertaining which of them is on the whole likely to be the most convenient in practice. This has at length inclined me rather to prefer the one in which the line for correction has only one position, whilst the lines of altitude change their places for each degree of distance. In it the parts of the various lines are better proportioned to suit the several distances, it is more applicable to the solution of other problems, and it also admits of a convenient mode of reading off the correction in terms of any horizontal parallax. I shall therefore describe more particularly that construction in which the same line of correction answers alike for every distance, and in which there is only one line for the moon's altitude, whilst the star's altitude takes a different line for each degree of distance.

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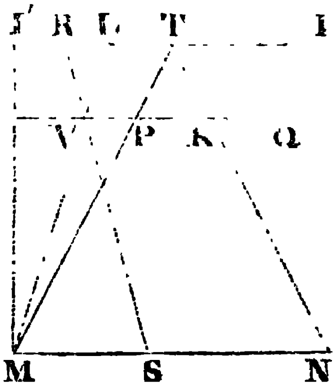
[*] Communicated by the Author.



Let M N as formerly be a line of sines whose zero is at M, and parallel to it draw I T equal the horizontal parallax; divide I T into a line of sines beginning from T, and produce it to contain another equal line of sines T I′, also beginning at T, but in the opposite direction.

Now to find the position of the line of star's altitude for a given distance: Lay a ruler from the sine of the distance taken on M N to its cosine reckoned on T I′; then through the point P where the ruler cuts M T draw P Q parallel to M N, and it is the position required.

Next to find the divisions on P Q for the sine of each degree of the star's altitude: Imagine the corresponding distance to be in a vertical circle, and lay a ruler from the sine of the moon's altitude on M N to the cosine of her altitude on I′ T if the effect of parallax is additive, or if the distance exceed 90°, but on I T when that effect is subtractive and the distance also less than 90°: the ruler will then cut P Q in the point that is to be marked with the intended degree of the star's altitude.

This latter precept obviously comprehends the preceding one, and the investigation formerly given is applicable to both; for by this construction, when N T is joined, it cuts any line of star's altitude P Q in a point K, such that $\frac{PK}{PQ} = \cos d$. So that, retaining the former notation, if S V M V be drawn and produced to meet I I′ in R and L, we have just the same state of things as before. In this arrangement, the lines of star's altitude for distances greater than 90° evidently lie to the left of M T; and the corresponding parts of such a line for any distance above 90° are just equal to those belonging to a distance as much under 90°; both being in one straight line, and reckoned from the same point of M T, although in opposite directions. If I′M be joined, it forms the limit of any line of star's altitude that lies to the left of M T: since the part that would fall without I′M can never come into use; because both I M and I′ M cut every line of star's altitude in the point which answers to sine of the distance. This form of construction is therefore more compact than the one formerly described.

When the distance is less than 90°, the additive correction lies to the left of T, and the subtraction to the right; but if the distance is not less than 90°, the correction is always subtractive. This correction for parallax may be obtained nearly as directed in the former paper, by laying a ruler from the moon's altitude on M N to that of the star on the line belonging to the distance: the segment of I I′ intercepted between the ruler and T will then be the correction in terms of I T.

If I T = 60°, the correction may easily be reduced by practice to suit any other horizontal parallax, especially since the number 60 contains such a variety of divisors, and is besides the radix of the sexagesimal scale;—a circumstance which renders it more convenient for the purpose than any other number.

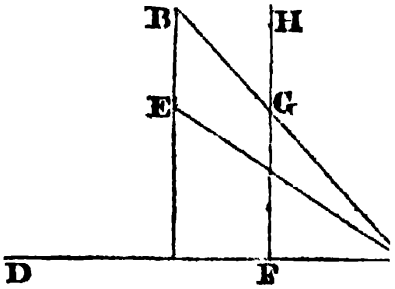
But perhaps the following would be an easier though less accurate mode of reading off the correction at first in terms of any horizontal parallax:—Draw as many straight lines, all equal and parallel to I I′, as there are horizontal parallaxes differing by one minute: cross these again by another set of parallels drawn pretty close to each other, and divide each of the first set into as many equal parts as there are minutes in the horizontal parallax which it represents. Then having applied the ruler to the altitudes and distance, as already directed, follow one of the cross parallels from the point where the ruler cuts I I′, and it will cross the line representing the given horizontal parallax in the point which indicates the corresponding correction. In this method I T should be made either the greatest or least horizontal parallax.

When it is thought necessary, the final effect of parallax may be allowed for in the principal effect as follows: Let the moon's altitude be half corrected for parallax, and therewith find the effect of parallax on distance nearly; then, without removing the ruler, apply half the effect just found to the distance; and by shifting the ruler a very little to suit the distance so corrected, it will give the true effect of parallax. The parallax in altitude may be readily had on I T, &c. by entering with the moon's zenith distance as an argument.

The

The construction that has now been described might assist in giving a solution in round numbers of various problems in spherical trigonometry. Thus, having given the latitude of the place, the declination and altitude of the sun to find the time; in place of the distance, the altitudes of the moon and star, substitute the colatitude, the declination and altitude of the sun respectively; and by attending to the signs of the quantities, we shall have cos. declin. ×, cos. horary angle. This again might be freed from cos. declin. by a small separate scale, or by such a method as that proposed for the different horizontal parallaxes.

I have also succeeded in materially improving the construction, formerly proposed for showing the effect of refraction; and in addition to other advantages, it does not now require a ruler laid across in using it.



Let A B be a line of sines beginning at A; draw D A C perpendicular to it, and join B C; draw also a set of straight lines parallel to A B, and reaching from A C to B C: next lay a ruler from D through each degree of A B, and draw as many straight lines extending from A B to B C; these will evidently divide each parallel into a portion of a line of sines, but always to a greater radius, as they are more distant from D. From C draw a set of diverging lines to meet A B, and of course dividing all the other parallels in the same ratio as they do A B. Hence if the greater altitude be sought out on A B, and if from that point E an oblique line be followed till it meet the less altitude on some other parallel F G, it is plain that every oblique line drawn from C must also cut A B and F G in points which denote two altitudes whose sines are in the same ratio as those of the first two. If therefore G H be the effect of refraction in the first case, it will be so for the same distance whenever the sines of the altitudes are in the same ratio; that is, whenever the less altitude falls on F G. Reckoning then the greater altitude always on A B, it follows that the less of every two altitudes whose sines have the same ratio, must fall on the same parallel; or, that each parallel belongs to a different ratio of the sines of the altitudes. So that a curve may be constructed in the manner formerly directed, of such a nature that when the several parallels are produced to meet it, the part of each produced, or the segment intercepted between B C and the curve may always represent the effect of refraction on the same distance, whenever the less altitude falls on that parallel.

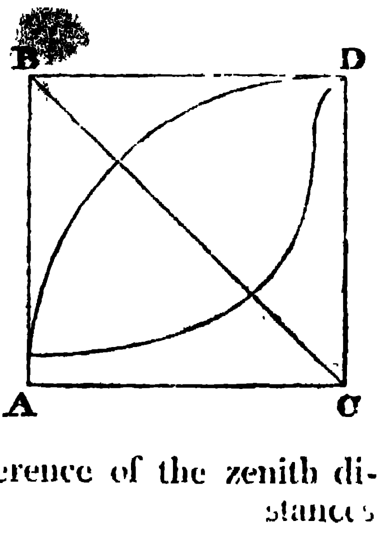
The use of a linear table constructed in this form for showing the effect of refraction, is therefore very simple: for having found on what parallel the less altitude falls, trace that upward, and its segment intercepted between BC and the curve corresponding to the given distance, is the effect required, which is always additive. This table is not quite half the size of that formerly described, when the corrections in both are on the same scale, and there is sufficient room for inserting it above MN in the table for parallax. If small altitudes are not to be used, a considerable part of the figure next AC might be omitted, which would still further reduce the size.

It was already remarked that on the same distance, the effect of refraction is nearly the same for any two altitudes whose sines have the same ratio; and to render the above explanation more simple, I only introduced lines of sines. But when the sines are proportionals, their like powers or roots are also proportionals; and therefore the effect of refraction must be nearly the same when the like powers or roots of the sines have the same ratio. Hence, if in place of lines of sines we use their square roots the above construction would be much improved: for by this means the curves being less crowded would meet the parallels much less obliquely when the effect of refraction is great; and thus the different parts of the scale from which the correction is to be read would be better proportioned. But I have not yet ascertained what root of the sines would answer best. It was however to effect the same end in some degree, that the lines were to be drawn diverging from D, and not parallel to AC.

I shall now proceed to describe the outline of a very different contrivance for clearing the distance; if this has not every property that the other possessed, it is much more easily constructed, it does not require a ruler laid across, and when of the same size as the former it shows the correction for parallax on twice as great a scale.

In such a spherical triangle as is formed by the distance and the complements of the altitudes, it is in effect demonstrated by almost every writer on spherics, that a perpendicular from the zenith being let fall on the distance, divides it into two segments whose cosines are as the sines of the altitudes; and also, that the tangents of these segments are respectively proportional to the cotangents of the altitudes multiplied by the cosines of the adjacent angles at the base: that is, nearly as the effects of the refractions on the distance. If, therefore, a ready method could be found for dividing the distance into the two proper segments, it would afford the means of obtaining the effect of refraction with considerable facility. This division has sometimes been accomplished by calculation, as in Witchel's method; and can likewise be done by projection; but at present, I wish to show how a very simple diagram may be constructed to serve the purpose, as well as to give the effect of parallax.

Let A D be a square of which the two sides A B, C D are lines of sines whose zeros are at A and C; the corresponding degrees being joined by straight lines which are of course parallel to each other; and these again are to be divided into sixty equal parts by a set of straight lines parallel to A B, which in their turn are divided by the former into lines of sines the same as A B. The lines parallel to A C are also cut by a curve in such a manner that the segment of each reckoned from D C may represent the refraction corresponding to that altitude, and the same lines are further cut by a circle described from A to D about the centre C, so that the segments will always represent the parallax in altitude for a horizontal parallax of 60′: each of the 60 equal parts being counted one minute of parallax; but to enlarge the scale as much as possible, it is proposed to reckon each of the equal parts to be only 10″ of refraction, and the whole line only ten minutes. From C draw a set of diverging lines to spread over the whole square; these will obviously cut the vertical lines proportionally, so that if the segments of two vertical lines cut by one oblique line be the sines of the altitudes, the cosines of the segments of the distance will also be segments of the same verticals when both are cut by another oblique line, since these cosines are proportional to the sines of the altitudes.



To find the effect of refraction by this method: Look for the greater altitude in A B, and from that point follow down an oblique line till it meet the less altitude in some other vertical; then follow up at same time the two vertical lines in which the altitudes were found till some oblique line cut them both in such a manner that the sum or difference of the *remaining* zenith distances may equal the distance; and the sum or difference of the corresponding refractions will be the effect required, which is always additive. There is no ambiguity as to whether the sum or difference of the zenith distances is to be taken; because only one of them (the sum or difference) can be equal the distance, unless when one of the remaining zenith distances is nothing; and the sum or difference of the refractions is to be taken according as the sum or difference of the zenith distances was used. The case that requires their difference is easily known in following up the altitudes, since the greater altitude will then seem to go above the top before their sum could be so small as the distance.

Next, to find the effect of parallax: Observe where the horizontal line on which the effect of the moon's refraction was found cuts the circle A D: from this point trace down an oblique line till it cut the parallel of the moon's altitude, and the segment of this parallel reckoned from C D, is the principal effect of parallax in terms of a horizontal parallax of 60′. For the tangent of segment of distance adjacent to the moon, is equal the cotangent of the moon's altitude multiplied by cosine of angle at the moon: and the quantity we have obtained is obviously equal to tangent of the above segment multiplied by sine moon's altitude; that is, to cosine of angle at the moon multiplied by cosine of her altitude. The above effect is easily reduced to suit any horizontal parallax by the common rule of practice, as was directed in the other method; and the final effect of parallax may be allowed for, when it is deemed necessary, by half correcting the altitudes and distance, and then repeating the operation to find the true effect of parallax; though this cannot be done quite so conveniently here as in the other method. The correction for parallax is additive, when the difference of the refractions is used and the moon at the same time the higher body. In all other cases it is subtractive.

It may be observed, that in place of a square, A B D C might have been an oblong, and A D the quadrant of an ellipse. Nor is it absolutely necessary that the vertical lines should be lines of sines, or lines of their powers; they might, on the contrary, be divided in any proportion that may be thought to suit better; but then the lines diverging from C could not always be straight lines, neither could A D be a circular ellipse. If, however, the rectilineal part of such a diagram as that already described were first constructed, it would be easy from it to draw another in which the vertical lines were divided in any other proportion. For having joined the like points of a different division in another set of vertical lines, transfer the divisions of the horizontal lines in the former figure to the corresponding equal lines in the new figure, and through these points draw curve lines which, in theory at least, will answer the same purpose as the straight lines radiating from C do in the other figure. The curve for refraction will be alike easily constructed in either figure; and the curve for parallax may always be laid down by help of a line of sines. In the same way it is easy to see how the vertical lines in the second diagram of the method described in the preceding part of this paper, admit of being divided in any proportion; as also how a portion of the triangle A B C in that figure might be expanded or contracted without using square roots of the sines.

I am, gentlemen,
Your most obedient servant,
Henry Meikle.

P.S.—It has frequently been remarked, that the accuracy of altitudes taken near the horizon is not to be trusted on account of the uncertainty of the refraction; and a similar objection though on a different ground has been raised against altitudes taken near the zenith. These remarks I do not mean to controvert; but it is no less true, that there are some cases of frequent occurrence, in which such altitudes although uncertain in themselves produce little or no uncertainty in clearing the lunar distance. When the distance is in a vertical circle, any error or uncertainty in altitude is apt to occasion a like error in distance. As the distance however deviates from the vertical position, the effect of an error in altitude will generally diminish; and when both altitudes are small and nearly equal, it would require a great error in altitude to affect the distance materially. Equal altitudes of any magnitude may thus be used without much danger of error. But it might easily be shown that, when both objects are in the horizon, the effect either of uncertainty in refraction, or of refraction itself, is not sensible on the distance: hence the observed horizontal distance if near to 90° may be safely used as the true; because the effect of parallax then vanishes likewise; and since the effect of parallax on the horizontal distance, when that differs from a quadrant, may be so readily had from the 13th of the Requisite Tables, every opportunity should be embraced of measuring the lunar distance when both objects are in the horizon.

The foregoing graphical methods are scarcely suited to very small altitudes; but still they may approximate to the truth when the altitudes are small and nearly equal, if the effect of refraction which they give be divided by the number of degrees +1 contained in both altitudes: then the quotient being subtracted from the above effect will leave the true effect of refraction pretty nearly.

In taking an altitude very near the zenith for clearing the distance, it is sufficient if we can only be sure of its being within two degrees of the zenith, provided the lower altitude and the distance (not less than 20°) be properly observed; for if to the lower altitude we add the distance, the sum or its supplement may for ordinary purposes be used instead of the real apparent altitude of the higher body; and the reduction of the distance in such cases, will be the sum or difference of the corrections in altitude. But I do not mean to say that this obviates the effects of the spheroidal figure of the earth; nor have I endeavoured to determine how far the effects of small uncertainties in altitude might be obviated; at present I only meant to bring some cases which appeared to be neglected into more general notice.

LIII. *On some new Tables of Aberration and Nutation.* By FRANCIS BAILY, *Esq. F.R.S.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—HAVING been engaged, a short time since, with my friend Mr. Benjamin Gompertz, in investigating the principles of *Aberration* and *Nutation*, with a view to the formation of more correct tables on the subject, it occurred to us that an improvement might be made in the arrangement of the arguments of the usual tables for finding these quantities.

If we denote the obliquity of the ecliptic, the right ascension and declination of the star, the true longitude of the sun, the true longitude of the moon, and the mean longitude of the moon's node, by ω, Æ, D, $\odot$, ☽ and ☊ respectively, we shall have the following correct and general formulæ for determining the above-mentioned quantities.

Right Ascension.

1. $$-20'',2550\,(\cos\omega\,.\,\cos\text{Æ}\,.\,\cos\odot+\sin\text{Æ}\,.\,\sin\odot)\sec D$$

2. $$\begin{cases}-\;1'',2255\,\sin 2\odot\\-(0'',5799\cos\text{Æ}\,.\,\cos 2\odot+0'',5319\sin\text{Æ}\,.\,\sin 2\odot)\tan D\end{cases}$$

3. $$\begin{cases}-15'',3958\,\sin\text{☊}\\-(8'',9771\cos\text{Æ}\,.\,\cos\text{☊}+6'',6821\sin\text{Æ}\,.\,\sin\text{☊})\tan D\end{cases}$$

4. $$+\;0'',0877\cos\text{Æ}\,.\,\cos 2\,\text{☊}\,.\,\tan D$$

5. $$\begin{cases}-\;0'',1846\,\sin 2\,\text{☽}\\-(0'',0874\,\cos\text{Æ}\,.\,\cos 2\,\text{☽}+{\cdot}0801\sin\text{Æ}\,.\,\sin 2\,\text{☽})\tan D.\end{cases}$$

Declina-

Declination.

$$1.\ \left\{\begin{array}{l} +20'',2550\,(\cos\omega.\,\sin\text{Æ}.\,\cos\odot-\cos\text{Æ}.\,\sin\odot)\,\sin D \\ -20'',2550\,\sin\omega.\,\cos\odot.\,\cos D \end{array}\right.$$

$$2.\quad +\ 0'',5799\,\sin\text{Æ}.\,\cos 2\odot-0'',5319\,\cos\text{Æ}.\,\sin 2\odot$$

$$3.\quad +\ 8'',9771\,\sin\text{Æ}.\,\cos\text{☊}-6'',6821\,\cos\text{Æ}.\,\sin\text{☊}$$

$$4.\quad -\ 0'',0877\,\sin\text{Æ}.\,\cos 2\,\text{☊}$$

$$5.\quad +\ 0'',0874\,\sin\text{Æ}.\,\cos 2\,\text{☽}-0'',0801\,\cos\text{Æ}.\,\sin 2\,\text{☽}.$$

In each of these series, No. 1 denotes the aberration, No. 2 the solar nutation, and No. 3 the lunar nutation: No. 4 and 5 are generally omitted as being too small to affect the results. The coefficient of the aberration is that given by M. Delambre; and the coefficients of the nutation are those which have been deduced by M. Lindenau.

There are various modes of arranging these formulæ, for the convenience of computation: and Baron Zach has shown (in his *Nouvelles Tables d'Aberration et de Nutation*) that they may all be reduced to the following general expressions:

$$\begin{array}{l} M.\ \sin(\odot+N) \\ M'.\ \sin(2\odot+N') \\ M''.\ \sin(\text{☊}+N'') \\ \quad \&c. \qquad \&c. \end{array}$$

where N, N′, N″, &c. denote constant angles, and M, M′, M″, &c. the maxima of the quantities employed in the computation: and I believe it is in this way that most of the tables, now in use, have been computed. As far as the aberration only is concerned, this expression might answer every useful purpose, and be nearly as convenient as any other. But when the quantities depending on the nutation are involved, they become so numerous and complex, that the finding of the value of M. and N. is exceedingly troublesome: and, indeed, when these values themselves are given in tables, the computation, for any particular case that may arise, requires to be conducted with some care and address.

Tables of aberration and lunar nutation, for several of the principal stars, are to be met with in many astronomical works: but I am not aware that any tables have yet been formed for the *solar* nutation. Nevertheless the solar nutation is capable of being combined with the lunar nutation, and of forming one general expression therewith: and it was in this manner that Mr. Gompertz and myself proposed to treat it. For it is evident that, in the case of *right ascensions*, the quantities 2, 3, 4, and 5 above mentioned, may be denoted by the following general expression: viz.

$-(15'',3958$

$$-(15'',3958 \sin \text{☊} + 1'',2255 \sin 2\odot + 0'',1846 \sin 2\text{☽}) \times (1 + \tan \omega . \sin \text{Æ} . \tan D)$$

$$-(8'',9771 \cos \text{☊} + {\cdot}5799 \cos 2\odot + {\cdot}0874 \cos 2\text{☽} - {\cdot}0877 \cos 2\text{☊}) \times (\cos \text{Æ} . \tan D).$$

And that, in the case of *declinations*, we have only to change the right hand factors for ($\tan \omega . \cos \text{Æ}$) and ($-\sin \text{Æ}$) respectively.

Whilst this subject was under our consideration, (and after I had entered on the computation of the tables, as a specimen of the plan,) the fourth number of M. Schumacher's *Astronomische Nachrichten* appeared: in which M. Bessel has pursued nearly the same course of investigation as that to which I have just alluded: with this additional improvement, however, that the correction for the *precession of the equinoxes* is introduced without causing any additional trouble to the computer. And my object, in now making this communication, is to call the attention of the public to this new plan, which possesses many advantages over those which have hitherto been in general use: and which, by the further labours of M. Bessel (to which I shall presently allude) are rendered of very extensive application. M. Bessel's corrections are as follow:*

Right Ascension.

$$+(46'',0175 + 20'',0436 \tan D . \sin \text{Æ})\, t$$
$$-(15'',3958 + 6'',6821 \tan D . \sin \text{Æ}) \sin \text{☊}$$
$$-(\ 1'',2255 + 0'',5319 \tan D . \sin \text{Æ}) \sin 2\odot$$
$$-(\ 8'',9771 \cos \text{☊}\,\dagger) \tan D . \cos \text{Æ}$$
$$-(\ 0'',5799 \cos 2\odot) \tan D . \cos \text{Æ}$$
$$-(18'',5837 \cos \odot) \cos \text{Æ} . \sec D$$
$$-(20'',2550 \sin \odot) \sin \text{Æ} . \sec D.$$

Declination

$$+20'',0436 \cos \text{Æ} . t$$
$$-\ 6'',6821 \sin \text{☊} . \cos \text{Æ}$$
$$-\ 0'',5319 \sin \text{[illegible]}\odot . \cos \text{Æ}$$
$$+\ 8'',9771 \cos \text{☊}\,\dagger . \sin \text{Æ}$$
$$+\ 0'',5799 \cos 2\odot . \sin \text{Æ}$$
$$-(18'',5837 \cos \odot) \times (\tan \omega . \cos D - \sin \text{Æ} . \sin D)$$
$$-(20'',2550 \sin \odot) \cos \text{Æ} . \sin D.$$

The first line in each of these expressions denotes the precession of the equinoxes, where t is equal to the time elapsed

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[*] Some of the co-efficients here given differ, in a small degree, from those given by M. Bessel; there being evidently an error in his computations; as I have more particularly remarked in my "Astronomical Tables, &c. for 1822."

[†] M. Bessel has here introduced the quantity (${\cdot}08768 \cos 2\text{☊}$): but I have rejected it from this view of the subject, as too small to be of any importance.

from the commencement of the year, expressed in the decimal part of the year.

In order to render these expressions more convenient for computation, let us pursue M. Bessel's plan, and make

$$\frac{6{,}6821}{20{,}0436} = 0{,}3334$$

$$\frac{0{,}5319}{20{,}0436} = 0{,}0265$$

whence we shall have

$$46{\cdot}0175 \times 0{\cdot}3334 = 15{\cdot}3412 = 15{\cdot}3958 - {\cdot}0546$$

$$46{\cdot}0175 \times 0{\cdot}0265 = 1{\cdot}2212 = \ 1{\cdot}2255 - {\cdot}0043$$

and by proper substitution the preceding expressions may be reduced to the following ones:

Right Ascension.

$$+ (t - {\cdot}3334 \text{ sin } \text{☊} - {\cdot}0265 \text{ sin } 2 \odot) \times$$
$$(46{,}0175 + 20{,}0436 \text{ tan D. sin Æ})$$
$$-(8{,}9771 \text{ cos } \text{☊} + {\cdot}5799 \text{ cos } 2\odot) \text{ tan D. cos Æ}$$
$$-18{,}5837 \text{ cos } \odot. \text{ cos Æ. sec D}$$
$$-20{,}2550 \text{ sin } \odot. \text{ sin Æ. sec D}$$
$$-{\cdot}0546 \text{ sin } \text{☊} - {\cdot}0043 \text{ sin } 2\odot.$$

Declination.

$$+(t - {\cdot}3334 \text{ sin } \text{☊} - {\cdot}0265 \text{ sin } 2\odot)\ 20{,}0436 \text{ cos Æ}$$
$$+(8{,}9771 \text{ cos } \text{☊} + {\cdot}5799 \text{ cos } 2\odot) \text{ sin Æ}$$
$$-18{,}5837 \text{ cos } \odot \ (\text{tan } \omega. \text{ cos D} - \text{sin Æ. sin D})$$
$$-20{,}2550 \text{ sin } \odot. \text{ cos Æ. sin D.}$$

It is evident that the two quantities, in the last line of the expression for the right ascension, are too minute to affect the result in any sensible manner, and may therefore be safely omitted. Whence, if we make

$$A = +t - {\cdot}3334 \text{ sin } \text{☊} - {\cdot}0265 \text{ sin } 2\odot$$
$$B = -8{,}9771 \text{ cos } \text{☊} - {\cdot}5799 \text{ cos } 2\odot$$
$$C = -18{\cdot}5837 \text{ cos } \odot$$
$$D = -20{\cdot}2550 \text{ sin } \odot$$
$$a = +\ 46{\cdot}0175 + 20{\cdot}0436 \text{ tan D. sin Æ}$$
$$b = + \text{ tan D. cos Æ}$$
$$c = + \text{ cos Æ. sec D}$$
$$d = + \text{ sin Æ. sec D}$$
$$a' = +\ 20{\cdot}0436 \text{ cos Æ}$$
$$b' = - \text{ sin Æ}$$
$$c' = + \text{ tan } \omega. \text{ cos D} - \text{sin Æ. sin D}$$
$$d' = + \text{ cos Æ. sin D}$$

we shall have the total correction for

$$\text{Rt. As.} = Aa + Bb + Cc + Dd$$
$$\text{Dec.} \quad = Aa' + Bb' + Cc' + Dd'.$$

It is evident that the quantities denoted by a, b, c, d and a', b', c', d' are *constant* for each star; and that the quantities C and D are *common to every star:* whence, tables of these values, once computed, will last for many years without requiring any correction.

It is in this manner that M. Bessel has arranged the tables which he has recently published in M. Schumacher's *Astronomische Hülfstafeln* for 1822 *. The values of C and D are computed for *every day* in the year: and the values of a, b, c, d and of a', b', c', d' are computed for *upwards of five hundred of the principal stars;* the whole of which are contained in about forty octavo pages. A and B must be computed for *each* year; and M. Bessel has given the values for every tenth day in the years 1819–1822. The whole of the tables are calculated by MM. Rosenberg and Scherke, two distinguished pupils at the observatory at Königsberg.

A very slight examination of this method will show the great advantage which it possesses over that of Baron Zach, for occasional reference. By the latter, we have to make a separate computation for the precession; then we have to form three distinct arguments for the sines; the logarithms of which are to be sought; and after having proceeded thus far (not forgetting that there are no tables for determining the argument for the solar nutation) we have just as much to do as is required by the whole of M. Bessel's method: viz. to take the sums of four logarithms and find their natural numbers. But there is this additional advantage and convenience in M. Bessel's mode,—that all the logarithms are found at two openings of the book; and any particular case may thus be readily solved with the help only of a small table of logarithms. Whereas, by Baron Zach's mode (and indeed by every other mode with which I am acquainted) a reference must be made to *many* works before a correct solution can be obtained.

I have stated that the values of A and B must be computed for each separate year: but the labour of such computation may be abridged in the following manner. Make

$$\alpha = -{\cdot}0265 \sin 2\odot + t$$
$$\beta = -{\cdot}5799 \cos 2\odot$$

then we shall have

$$A = \alpha - 0{\cdot}3334 \sin ☊$$
$$B = \beta - 8{\cdot}9771 \cos ☊$$

But α and β are the same for each successive year, and therefore the computations, when once made for any particular days in one year, may be considered as constant quantities for the same days in any succeeding year. Whence, the only variable parts of these expressions will be those which depend on the sine and cosine of ☊; which will vary little during the space of a hundred days. So that it will only be necessary to compute *four* values of each for any given year; and to deduce the values for the intermediate days, by interpolation. Supplementary tables of this kind would have formed a valuable addition to those which M. Bessel has given. It would also have been a considerable improvement, if the values of *a*, *b*, *c*, *d*, had been divided by 15 so as to have given the corrections *in time*, instead of *in arc*.

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* This work may be procured of Messrs. Treuttel and Wurtz, Soho-square. It would be rendered of more general use in this country, if an English preface were prefixed.

With respect to the few stars which are observed at Greenwich, and which are called (*κατ' ἐξοχὴν*) *Greenwich stars*, M. Bessel has formed *other* tables, more convenient for finding their *daily* corrections. The constant use which is made of those stars, in an observatory, requires that *their* corrections should be given for *every day* in the year. But it would be an endless task to give *such* corrections for the whole of the 500 stars which I have alluded to in this letter. It was desirable therefore that a convenient mode should be given for deducing those values, as occasion might require. Such a mode is now presented by M. Bessel: and the astronomical world is much indebted to him, and his distinguished pupils at the observatory, for this proof of their zeal and public spirit. It is gratifying to see the patronage and protection which are afforded to young men of science on the continent, and to view the rising talents of those who are ambitious to pursue the steps of their illustrious predecessors. Every attempt is encouraged which tends to relieve the practical astronomer: and France, Italy, Portugal, Germany, Russia, Denmark and Prussia have each in their turn set a laudable example, to this country, of the good effects of this fostering spirit; which, however, *we* seem very loth to pursue.

I am, gentlemen,
Your obedient servant,
Gray's Inn, Oct. 18, 1822. FRANCIS BAILY.

P.S.—On referring to M. Schumacher's *Astron. Hülfs.* for 1820, it appears that M. Bessel's mode is similar to that which has been proposed by M. Gauss. But, no explanation is there given of the steps of the process.

LIV. *D.'s*

LIV. *D.'s Second Reply to C. on Mr.* HERAPATH'S *Theory.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,— MY last clearly established, against Mr. Phillips's correspondent C., some very inexcusable misrepresentations of Mr. Herapath. Instead of answering these charges candidly, he has endeavoured to divert the attention of his readers to the twisting and torturing of some of my expressions for the mere purpose of trying to retaliate. After he has thus consumed three or four months, and has carefully compared the whole of my quotations, examined the position of every word, and calculated the propriety of every comma; he has at length, amidst his amplified exaggeration, been obliged to acknowledge, in one of his powerful instances, that "as the *tone* and *emphasis* of the sentences are changed, rather than the *sense*, it is *not* of *material* consequence!"

C., Annals for December, p. 423, says, "How Mr. H. proves 'that the intensity of the stroke is the force with which each of the balls is acted on in a direction opposite to that in which it came at the time of the contact,' I am at a loss to discover." "The *intensity* of the *force* is 'equal to the *sum* of the momenta' 'with which *both balls* come in contact.'" In quoting the substance of the last of these sentences, Annals for May, p. 358, "I observed," C. says, "that the *intensity* of the *stroke* between two bodies moving towards opposite parts is equal to the *sum of their momenta.*'" Surely nothing can be plainer or fairer than this quotation, particularly when all I have said in the same paragraph and preceding page be considered; yet C. in his reply, Annals for September, pp. 207 and 208, flies out into a violent philippic, and declares, because I had not written "*equal balls*" and "*equal momenta*," that my quotation "*is absolutely false*," and a "*wilful* misstatement." When I first read these fearful words, I was surprised at the charge; but when I came to examine the real groundless foundation of them,

> "Obstupui, steteruntque comæ, et vox faucibus hæsit."

For it is not the least curious part of this affair, that in the whole paragraph, upwards of a page and a quarter, which contains C.'s much-injured sentence, he has not once used the term "*equal balls*," or "*equal momenta*," except in two quotations from Mr. Herapath, both a long way before the sentence in question. So great a reason therefore has C. to complain, that the misrepresentation, if there were any, would have been of Mr. H. rather than of himself. However, if in this matter we cannot perceive the justice of C.'s complaint, we must at least admire his ingenuity; for so cleverly has he drawn his case, that any one who reads that alone would be apt to cry out with Demosthenes, "Now indeed do I hear the voice of one that is injured."

Page 201 of C.'s reply presents us with another instance of this kind, much too pleasant to pass over in silence. "Experiment," says C. in his first attack, p. 419, "has clearly shown that *caloric*, or the immediate cause of heat, whatever it may be called, *cannot be destroyed.* However, under *particular circumstances, it may become for a time imperceptible, it can again be developed*, and so be shown to have continued its existence: if, therefore, heat and motion be identical, motion cannot be destroyed." A plainer allusion than this to the disappearance and reappearance of heat in the changes of state, and a clearer objection to the theory of heat by motion, from its assumed inability to explain such phænomena, could hardly be expected from this writer. As such no doubt it would have been allowed to pass, and have been ranked amongst C.'s unanswerable objections, had I not unhappily shown that C., before he published this, had seen the Number containing Mr. H.'s theory of the changes of state, his mathematical computations of the various dependent phænomena, &c., and consequently that C.'s observations were a gross attempt to misrepresent. Finding however the case a little too clear against him, C. turns round in his reply, and by a long list of unqualified "I never dids" and "I did nots" boldly denies both object and allusion; but unfortunately in the close admits, he "intended to show that the indestructibility of caloric," in the changes of state of course, "is a strong argument to prove it *cannot be* merely *motion*." As a finale to such a novelty of consistency, C. should have added: "Mallem mori quàm mutare."

The liberal use C. makes of the polite terms "false," "absolutely false," &c. I should have left unnoticed, had he not made such elevated expressions the agents for depriving his own character of the imputation of a dishonest disputant to fix it on mine.

That I have held up to ridicule absurdities legitimately drawn from his notions, is true. This however is not my fault, but his. If a man be so weak as to meddle with subjects above his reach, and imprudent enough to publish his crudities, he must expect to be laughed at. As to making "false" quotations for the purpose of wilful misrepresentation, I have not done it, and this C. well knows. Nor has he, with all the assistance of invention, and the wide latitude which the numerous and in some instances nonsensical deviations of the printed from the MS. reply have afforded him, been able to make out one such a case. This, in the present state of C.'s writings, is all the notice I can take of his baseless accusation.

Every person, who takes the pains to read C.'s last communication, can easily discover the "honourable" means to which he has had recourse to shift from the charges of palpable misrepresentation which I have in my former reply so clearly proved. Before I proceed "to examine the reasoning" which he calls mathematical, I will extract from C.'s "reply" a few specimens out of a countless number of that sort of truth, "which," one would have thought, "his honourable feelings, intelligence, and integrity, would have alike disdained." If they can do nothing else, they will serve to show how impossible it is for a person of C.'s "integrity" to depart from that kind of rectitude to which he has been accustomed. In the Annals for September, p. 198, C. giving a quotation from my first reply makes me say, "In more than one instance C. has not been over delicate in this respect." The original has the phrase "*I think*" immediately after "*has*." But this omission, which merely converts a matter of opinion into an *absolute* assertion, is, I presume, in C.'s system of no consequence, especially as it happens to be in the very page and paragraph, in which he is trying to call in question my "moral deficiency" for a misrepresentation of which indeed his own imagination is the author. I pass over in the same pages his "honourable" suppression of the principal part of what I had (Annals, April, p. 292,) advanced to prove his having "falsely" charged Mr. H. with attributing to hard bodies the properties of elastic, and his insinuating that what he has quoted is all that I had advanced; his curious quotations from Newton and Hutton to support a consequence of a property, instead of the property itself; his creditable quibble on the word *almost*; his very justifiable statement that D. "admits that Mr. H. advisedly used the one word instead of the other;" his edifying attempt to get rid of his discovery of having proved truth error; together with many things of the same stamp in the first few pages only of his reply, — not because I think they would be unimportant or not discreditable to a person who values his reputation, but because they appear in fact, in C.'s reply, like the glimmering of nebulæ stars amidst an endless constellation of glowing violations of facts; I pass, I say, these things over, not for their real but their relative unimportance. I will now, however, adduce a few specimens, that will not want the aid of comment to illustrate their object and their origin. They will besides set at rest the claims of C. to veracity, to "integrity," and to "honourable feelings." "But what has this to do," says C., Annals, September, p. 211, "with

Mr. H.'s proposition, '*that the* VELOCITIES *of the moving bodies have no effect on the* INTENSITIES *of the strokes?*' " Let us here observe that the part of the quotation in Italics is what C. has given between double inverted commas, and therefore intended to be a verbatim, I presume, transcript of one of Mr. Herapath's "propositions." The fact however is, there is no such a thing in words, sense, or import; and not merely in Mr. H.'s propositions, but not even in any part of his printed writings that I can perceive. His first Prop., which seems to be the only one relating to this subject, says: "*If two bodies absolutely hard impinge on one anot*[illegible]*the* DURATION *or* SMARTNESS *of the stroke is independent of the velocity of contact.*" This is all that he says of this subject in his "*propositions*;" and in a Cor. to the first Prop. he says: "Hence we gather, that in perfectly hard bodies the intensity of the impulse depends on the violence or momentum of contact, and is independent of the velocity of contact, *except inasmuch* as it is augmented or diminished by *that velocity*." Here, therefore, C. not merely gives to Mr. H. words which he never used, and a "proposition" which he never wrote, but actually a meaning totally different from any thing he ever published; and this for the mere purpose of creating a groundless opposition between Dr. Hutton, Prof. Playfair, and Mr. Herapath. If C. can edge out of this without a frank avowal of "wilful misrepresentation," let him; I shall be happy to see his manœuvres.

So much at home does C. appear in misrepresentations, and so easily and rapidly do they flow from him, that, even after he had indulged in the above glaring specimen, which one might have thought would last him through a few pages at least, he could not finish the subsequent paragraph in the same page without presenting us with another. As this is however one of C.'s ordinary aberrations from truth, I will leave it without further comment. In the very next page C. informs us, "D. says that the *velocity* of B after the stroke is $= \frac{A a B}{A+B}$." Turning to p. 362, Annals for May, from which C. pretends to have extracted this idea, I find I said: "the *motion* it" (that is, the *momentum* B) "acquires by the stroke $= \frac{A a B}{A+B}$." In the former case of misrepresentation C. put in Mr. H.'s mouth "*intensity* of the stroke" for "*duration* of the stroke," that is, *violence* for *time*; and he has now thought proper to mistake for me *velocity* for *momentum*; and this at a time when he is trying to demonstrate a paradox I had proposed. Of course his demonstration must be very complete and very unique.

One more example shall be all of this kind with which I will trouble your readers. At p. 406 of his first paper Mr. Herapath, among a variety of other numerical comparisons of his theory with experimental facts, mentions the results of some calculations on the mixtures of mercury at given temperatures and in given proportions. In all that he has here said of the subject, he has not so much as hinted at his method of deducing the principles on which he founds his calculations from his theory; as any one who chooses to take the trouble of turning to the place may convince himself. Mr. H. has indeed rested satisfi[illegible] this place with giving a simple statement of the results of his calculations, without once adverting to his mode of theoretical deduction; but in the third Prop., and its scholium of his second paper, he has entered into both theory and calculations at length. Now it is a circumstance worthy of observation, a fact highly illustrative of C.'s "honour" and "integrity," that he has omitted even to allude to the theory in Mr. H.'s second paper; and has quoted from the first the simple results, the mere calculations of the case in question, as a specimen of the "mode of reasoning," which he calls "reasoning in a circle," that Mr. Herapath pursues, for the purpose, I suppose he means, of misleading his readers.

To amplify these examples of C.'s regard to truth, which I have selected from a multitude of the same kind, is impossible. However, as there may be those who would hardly admit there can be a person that could in the face of facts, with unblushing confidence, and amidst the most palpable evidence to the contrary, wilfully be guilty of such misrepresentations as I have adduced, they may in charity be inclined to attribute them to a confused intellect. They may imagine, and account for these things by supposing, that C. cannot clearly discriminate between opinion and assertion, between one expression and another, between time and a blow, between momentum and velocity, or between narration and argument; and such ideas they may confirm by showing, that C. confounds effects with causes, calls his own *simple* statements reasoning, and tells us, as his own discovery I suppose, that Newton gives his sentiments of heat and gravity in a manner which "affords *no pretence* to consider them his opinions!!" To such charitable proofs of a cloudy mind, of course I can say nothing; and I must, I presume, therefore allow, that what in another would be considered the effects of intention, the want of "honour" and "integrity," must in C. be the natural consequences of an unnatural intellect.

We have now said enough to give a tolerable idea of the confidence to which C.'s quotations and observations are entitled; and perhaps enough to enable the reader to form a pretty good notion of the laudable course C. has taken, as well as of the value of those unusual associations of words, or if C will have it so of ideas, which he would dignify with the name of mathematical arguments. That he might not however have to complain of unequal attention to different parts of his performance, I will just take the trouble to show that his reasoning is an elegant counterpart of his charges and quotations.

C. attempted to raise an objection, in his first paper, against Mr. Herapath's theory c[illegible]at on the supposition that the velocity of the particles in bodies of equal temperature being "in an inverse proportion" to their masses, the less particles may overtake and strike the larger, whilst the larger can never overtake the smaller. To answer this which is evidently assuming that the motions of collision are the mean motions (though C. has the "modesty" to say, in his reply, that no such an idea of mean motion can be founded on his), I examined the case when the particles are supposed to be divested of that corpuscular attraction which would affect the uniformity of their motion. With this supposition, which, as I said, corresponds to the "mean circumstances of the case," I clearly demonstrated the futility of C.'s objection. After this I briefly considered the irregularity occasioned in the motions of the particles by their mutual attractions; and hence showed that the larger particles may sometimes overtake and strike the less, which consequently disproved C.'s superficial assertion to the contrary. C.'s refutation of my arguments to this effect, consists in avoiding their force, and making a round declaration, that supposing the particles to have no attraction "is directly contrary to the other supposition that the particles" would "have limited paths," did they attract one another; and that without attraction "the particles would altogether fly off and be dissipated." These discoveries of the difference between attraction and no attraction, must, the reader will perceive, considering their connexion with heat and collision, have great weight against what I had said; and the difficulties under which they were made, their novelty, and their importance, would fully entitle C. to say,

> "Dicam insigne, recens, adhuc
> Indictum ore alio."

This being the substance of C.'s reply to my exposure of his absurd objections to the theory of heat embraced by Newton, I think it unnecessary to dwell upon them any longer; and I shall therefore proceed to consider his notions of collision.

"If," says C. (Annals for September, page 210), "a *hard* moving body A strike a *hard quiescent* body B in the lines of their centres of gravity, *the quiescent body yields to the stroke;* and this it must do, *lessening A's motion* and *increasing its own, until it shall have acquired a velocity equal to that of* A." Taking this palpably absurd proposition for granted, which C. proves by saying it is "too self-evident to require further illustration," it manifestly follows, that some time, however short it may be, is required for the accomplishment of these "*yields to the stroke,*" "*until it shall have acquired,*" &c. During this time, therefore, as each of the bodies C. tells us receives its change "of course not among its parts, but altogether," the two bodies have different velocities, though both are continually in contact, and moving one before the other; and the velocities are differently affected, the one being an "increasing," and the other a decreasing velocity. Surely C. must have something more than common confidence in his skill to prescribe such a dose of absurdity as this for the credulity of the world! Yet this dose,—absurdity, proposition, or whatever it may be called,—demonstrated so appropriately and profoundly by the luminous assertion that it is "too self-evident to require further illustration," is the base of C.'s fabric of collision,—the effort of intellect on which he prides himself over and over,—the magic spell which is to destroy, not merely what Mr. Herapath and D. have written, but what Wallis, Wren, Huygens, and Newton, have written!! Really, should another edition of the Annals be published, I would recommend C. to preface his paper with the following modest effusion of Horace:

"Nil parvum, aut humili modo,
Nil mortale loquar.—"

Besides the absurd conclusion, that of two perfectly hard bodies in contact the leading one moves slower than the following, which so obviously and immediately flows from C.'s strange proposition and still more strange demonstration, it would be very easy to exhibit other instances, under different views, equally as absurd and as obvious. For example: we might show, if C.'s proposition be correct, that during the stroke the aggregate momentum of the two bodies, supposed to be in contact and moving with a common velocity, is absolutely less than the aggregate momentum before or after the stroke; because before the stroke is completed the body B, by C.'s proposition, "is" continually "increasing its own motion." Here, then, this monstrous conclusion follows, namely, that the aggregate motion of the balls both diminishes and increases of itself during the stroke. Such is the legitimate consequence of a proposition, which to C. appears "too self-evident to require further illustration" than a mere enunciation.

C. perhaps will tell us he has distinctly said that "this effect," that is the yielding "to the stroke," "lessening" and "increasing" of motion "in hard bodies, is produced instantaneously." Two sentences after the propositional sentence I have quoted, he has indeed used the word "instantaneously;" but if he means by this word any thing beyond a very short space of time, he directly contradicts himself both in word and sense. What, for instance, can be understood by saying, that "B. *yields* to the stroke, and this it must do by *lessening* A.'s motion and *increasing* its own, *until* it shall have acquired," &c. except that the stroke requires in its communication the lapse of time, though that time may indeed be very short? But, however short that time may be, it affects not the refutation I have given; since, by the authority of Newton and other philosophers in their investigation of the laws of refraction and reflection of light, &c. any portion whatever of time in which an effect is, as C. here supposes, continuously produced, may be mentally divided into as many parts as we please; each of which has its corresponding motion or velocity. This is all I contend for, and all that is necessary to prove the absurdity of C.'s views. I should not indeed have touched on this "instantaneously," had it not been to prevent C. from taking refuge behind a word whose existence here, it would rather be a compliment to C. to say, appears more the result of accident than design.

The calculations into which C. has entered, page 210, are, to say the best of them, mere childish amusements. A fine proof truly it must be of his views to give calculations of the collision of bodies; which, with sensible magnitudes, have no existence but in the imagination, and which consequently cannot be found to form experiments with:—he might as well have attempted to show the quadration of the circle by computing the longitude.

On the principle whose absurdity I have just exhibited depends C.'s refutation of most of my theorems, and the solution of a paradox I had proposed in the old theory. Elegant and conclusive of course we must expect his refutation and solution to be, from such a beginning; especially when we take into account, from the specimens I have given, C.'s tactics and skill at misrepresenting and confounding.

To narrow the subject of controversy on collision as much as possible, I will, with C.'s permission, pass over the rest of his observations on collision, and confine myself to the one point in question; namely, the effect of one perfectly hard ball striking in the line of their centres of gravity another perfectly hard ball at rest. By the theories of Wallis, Wren, Huygens, and Mr. Herapath, these balls will not continue together after the stroke, which I think is clearly demonstrated in Mr. H.'s 3 Prop., Annals for April 1821, and in my Prop. C. Annals for May 1822. The theory of collision now commonly received, however, which C. advocates, assumes that the balls will continue together after the impulse. Of C.'s attempt to prove this, I have, I apprehend, fully demonstrated the failure. If he fancies he can disprove what I have written, let him; but let him confine himself to direct arguments on this point; and it will then, I hope, soon be seen if C. can support his pretensions. Should he also conceive that he can pursue a better course to arrive at his goal than the one he has chosen in his reply, I will freely allow him to abandon the latter and to adopt any other of fair intelligible argument that he pleases.

Modern mathematicians imagine they prove the non-separation of the balls after the stroke by merely telling us, that they cannot separate because they are unelastic. Now what has elasticity to do with absolute hardness? Bodies, to be elastic, must in the first place be soft, that is, must have a property in direct opposition to perfect hardness. To tell us therefore that two hard balls, striking one another, cannot separate because they are not elastic, is to say that they cannot separate because they have not a property, which can never be a concomitant of perfect hardness. This is neither more nor less than giving exclusively to elasticity a consequence, and refusing a like general consequence to hardness on the mere plea that hardness is not elasticity. We should be fully as much justified in saying, that if a body move in any direction by attraction, the same or another body could not be made to move in the same direction by any other means, as for instance by repulsion or impulse, because neither is attraction. Any person however who would gravely make the latter assertion, would run as much risk of being laughed at as Whiston for calculating the day and hour that his ideal comet poured from its tail the waters of the Deluge.

What can be a more prominent proof of the absurdity of the old doctrine of collision than the consequence it furnishes in the case of two perfectly hard, equal, quiescent balls, being similarly struck by two perfectly hard balls having equal momenta? Here all the balls are supposed to be perfectly hard, the quiescent bodies perfectly equal, the momenta which occasion the intensities of the strokes perfectly equal, and the manner of communicating the strokes perfectly similar; and yet by the old theory the strokes themselves are generally unequal.

If a greater violation of common sense, and of the principles of collision themselves, can be adduced than this is, I should be happy to see it.

A Prop. of this kind I had introduced into my reply (Prop. A.), and had demonstrated it clearly and legitimately. C. has attempted to refute this Prop.—but how? To invalidate the proof, C. thought it, of course, necessary to quote it. Now this proof consists of five connected dependent periods. C. in his quotation has omitted the first two, lopped off the connective dependence of the third, left out a material part of the fourth, and *in toto* suppressed the fifth!! After having done so much towards the refutation, he quotes from Mr. Herapath, "that *all the strokes* between perfectly hard bodies have *no duration* and are thence equally smart." From this he infers, what is "undoubtedly" true, that "the strokes are equally *smart with respect to duration under every momentum*." But he goes on, "and consequently it may, with just as much reason, be concluded, that the *momenta* of the moving bodies have *no effect* on the *intensities* of the strokes;" that is, because the *velocities* have no effect on the *duration* or *time* of the strokes, the *momenta*, he concludes without showing why, have no effect on the *intensities* of the strokes. This is the result of C.'s profound refutation. Certainly its evidence is so clear that the scientific world cannot fail of receiving it with non-resisting veneration; for it is well known the mass drawn into the velocity gives the momentum, and therefore by C.'s new system, I suppose, the mass drawn into the duration of the stroke, that is, into *no* time, will give the intensity. I cannot say that this is precisely C.'s method of generating functions; but as he has manifestly discovered the same relation between the intensity of a stroke and momentum, as between velocity and *no* length of time, it is natural to conclude that he generates intensity from nothing in the same way as momentum from velocity. Whether this really be the case C. can tell us. I am however inclined to think, that if he does not give us something like a proof of his inference, the world will be apt to precipitate his pretensions a long way below the zero of mediocrity.

Having now given, of C.'s efforts to establish and refute, a specimen of each, from which the success and value of the rest may be easily computed, I will briefly advert to his treatment of the quotations I have made from Newton, Playfair, Emerson, and Hutton. To do this I shall spend no time in commenting. I shall at once come to facts; out of which, in order to a further saving of time, I will select one, the perversion of which will sufficiently, and I think satisfactorily, show to what lengths a man may be carried by invention uncontrolled by "integrity" or "honourable feelings." C. distinctly charges me with having intentionally perverted the meaning of the above writers, with having made a disingenuous use of their words, quoted passages in support of Mr. Herapath's theory which have no relation to the subject, &c. The following Prop. which I have given, p. 360, Annals for May, and the quotation which I have introduced in the Cor. p. 361, for the expressed purpose of showing that the Prop. is compatible with the notion introduced into the old theory of collision, will prove the truth of these charges, and of course C.'s claims to credit and confidence.

Prop. B.	Emerson's Tracts, p. 13.
"If two perfectly hard, equal, and quiescent balls be similarly struck by any two other perfectly hard balls, the *intensities* of the impulses will have a ratio equal to that of the generating *momenta*."	"If a body striking another gives it any motion, *twice* that body striking the *same* with the *same* velocity will give it *twice* the motion, and so the *motion* generated in the other will be as the *force* of percussion."

"Except in the *absolute equality* of reciprocal attraction in the planets," I observed in my first reply, "which Newton deduced merely from analogy, and of which no proof whatever can be furnished, there is no one *phænomenon* in which Mr. Herapath does not perfectly agree with Newton." C. says, this "assertion possesses as little truth as modesty." This I will certainly allow if C.'s assertions are to be the standard of truth, and his conduct the criterion of modesty; but not if other people's are. For instance: in reciprocal attraction between different planets, who has proved, and how, that a certain quantity of matter of one planet attracts with the same force that an equal quantity of matter of the other planet does? Certainly no one; nor can it from any data yet known be done. Newton, Laplace, Lagrange, &c. have analogically supposed it to be so; but have never proved it. This I will venture to assert C. will not get any scientific man of respectable character to contradict. So far is it from being proved that the different planets attract in proportion to their respective quantities of matter, that some pretty plain facts can be advanced to show the contrary. Euler, for example, has experimentally demonstrated that glass acts upon light to refract it more powerfully at a high temperature than at a low; and Newton has found that "all bodies seem to have their refractive powers proportional to their densities, or very nearly," the temperature being the same; that is, proportional to their ordinary attractions. Assuming therefore that the relation between refractive and attractive powers holds good in any one body; and that when its refractive power is augmented or diminished, its attraction also is augmented or diminished, which is by no means improbable, we should say that as the further planets must be colder than the nearer, the attractions of the former ought to be less than those of the latter. In other words, the apparent densities of the planets, which are the measures of their attractions, should diminish as we recede from the sun;—a circumstance which experience proves.

Again, the mean distances of comets from the sun being much greater than of the planets, we should apprehend their attractions ought to be much less; which is actually the case. Laplace, Playfair, and others, say their actions are totally insensible. A comet in 1454 eclipsed the moon, yet it produced no effects on the moon's or earth's motion. One in 1472 came so near the earth as to sweep over 120 degrees in one day; and another in 1770 passed almost among Jupiter's satellites; yet neither of them deranged in the least sensible degree the motions of the bodies to which it so closely approached.

A difference has likewise been noticed by Mr. Herapath, Annals for June 1821, p. 411, between the observed and Laplace's computed annual equation to the moon's mean motion, which would seem to show that the earth attracts the moon more strongly when nearer the sun than when further; as, for instance, stronger in December than in June. Magnetic, electric, &c. attractions are also well known to be influenced by the temperature. So that on all hands there is the greatest reason to believe that "the *absolute equality* of reciprocal attraction in the planets," is not true, however confidently C. may pour out his unsupported assertions to the contrary.

I dislike quibbling comment on the meaning of terms; and therefore I pass over C.'s observations on Mr. H.'s differing from Newton on an imaginary case of bodies inclosed in a space and supposed devoid of all attractive tendency, which he attempts to palm on the world as a *phænomenon;* that is, (as Dr. Hutton observes,) as "an appearance in physics discovered by observation of the celestial bodies or by physical experiments." There are, however, some that would from this circumstance tell C. he has as much to learn of the meaning of scientific terms as of science itself, "before," as he has observed of Mr. H., "he can attain among scientific men that rank to which he seems to aspire."

C.'s explanation or apology for his "pushing case" is curious indeed. He quotes what he had written, which is a clear proof of charging Mr. H. "with confounding pressure with impulse;" and then he makes a kind of supplicatory appeal against my understanding what he knows he cannot excuse. This is odd enough, if it be satisfactory enough.

"The toil," says he, "of dissecting and exposing a vast mass of mis-statement and misrepresentation, is laborious, fatiguing, and disgusting." I assure him I find it so, and something more when coupled, as in both his papers, with one continued "mass" of trifling, false reasoning, and error.

"It is by no means extraordinary," C. tells us, "that Mr. Herapath should be able by his theory *plausibly* to explain many phænomena"; "it is to be expected that every theory should afford an explanation of some class of experiments or observations." In reply to this, I ask C. Is Mr. H.'s theory by any means confined to a single class of observations? Has he not extended it to the theories of gases, evaporation, and vapours: the doctrines of capacity and latent heat; the phænomena of attraction and cohesion; the changes of state, &c.? In all or by far the greater part of these subjects has not Mr. Herapath openly and mathematically demonstrated the laws and phænomena? Has he attempted to shift from a single experiment to which credit can be attached? or rather, has he not decidedly confirmed his results by the experiments of our best philosophers; and solicited a further examination by others, which he has candidly pointed out; and whose results from his theory he has unhesitatingly computed? Do the theorems of any other person agree better, or even so correctly with experiment? In fine, has there been a theory proposed at any time which combines more (I might perhaps with perfect truth say so much) simplicity and comprehensiveness with such minute precision and experimental fidelity? If there has, C. can name it, and instance the cases of its equality, or, perhaps, superiority. Probably C. has himself a theory more elementary and more general to propose, which may account for the virulence with which he has attempted to oppose Mr. H. Should this be the case, I shall be exceedingly glad to see it: though I must confess, I think the probability of such a theory from such a writer rests upon a rocking rather than a rocky foundation.

If C. really think Mr. Herapath's theory defective or inconsistent with facts, why not come immediately to a numerical refutation of it in some of those numerous instances which Mr. H. has advanced? Surely there are cases enough mentioned to choose with advantage, without constantly beating about general views and metaphysical difficulties, which can perhaps never be brought to the test of experiment. Besides, Mr. H. has in his theory of latent heat, the postscript to his second reply to X, and several other parts of his writings, calculated results which I should think it would not be so extremely difficult to try. Overturning these instances would afford C. the gratification—a gratification that would not be lightly valued—of upsetting "Mr. H. in his prophetic character." Would not such a course also be much more "honourable," much more scientific, and much less liable to the charge of "meanness" and turpitude, than indulging in general clamour, calumny, and misrepresentation?

C. has, it must not be denied, stated one numerical objection, preparatory, I suppose, to a general refutation. It is this, that Mr. Herapath's calculation of the composition of water differs from like calculations of some chemical writers. Mr. H. by his theory from the well known fact, that two in volume of hydrogen unite with one in volume of oxygen to form water, finds that two particles of oxygen unite with one of hydrogen. This result he has, as far as it can be expected from the experiments in their present state, confirmed by the observed capacities of Crawford, Annals for Sept. 1821, p. 211; and he has further shown, that it is possible by correct experiments of this kind to confirm or refute his views of the composition of water. Some chemical writers, however, have concluded, from the proportion (two to one) of the component volumes, that two particles of hydrogen unite with one of oxygen; and others, I believe, have thought that a particle of water consists of one of each. If you ask these writers for a proof of either of these proportions, they can give you none, nor any reason why it should be one with one, two with one, or ten with one, in preference to any other proportion. They tell us experiments prove that airs combine in simple multiples of volume, and other bodies usually in simple multiples of weight; from which they very justly conclude that the elementary particles likewise combine in certain definite numerical proportions; but what these proportions are, it is impossible they could tell us from the multiples of weight or volume alone, unless the relative number of particles in equal weights or volumes was known, which they all confess they have no data for determining. The precise proportion therefore of one or of two particles of hydrogen to one of oxygen, is purely hypothetical, without any foundation in experiment; and this C., if he has any knowledge of chemistry, ought to have known.

C.'s subsequent paragraph, producing calculations for reasoning, I have already considered. It is almost as fine a specimen of his "modesty" as of his knowledge.

"I have already proved," quoth C., "by extracts from his works, that on the laws of collision Sir I. Newton's opinions directly, *both in words and meaning*, contradict Mr. Herapath's." Has C. then, it may be asked, disproved D.'s quotations from Newton's Principia? No.—Has he shown these quotations to be forgeries? No.—Has he demonstrated that Newton never wrote such things either in "words or meaning?" No.—But he has immediately afterwards told us, on his own authority *alone* be it observed, that Newton has published his "Cogitationes" in a way which "affords no pretence to consider them his opinions;" that is, in plain language, Newton has published as his own thoughts things which he does not believe!!! This discovery of C., so complimentary to the understandings of the rest of the world for the last century, is a key to the whole. It unquestionably to C.'s ability

> "Exegit monumentum ære perennius,
> Regalique situ Pyramidum altius;
> Quod non imber edax, non Aquilo impotens
> Possit diruere, aut innumerabilis
> Annorum series, et fuga temporum."

In fact, it indisputably proves that either Newton or C. knows not what he writes.

Some pains have been artfully taken by C. to identify D. with Mr. Herapath. The reasons he has assigned would just as well suit C.; since from various parts of his communication he appears fully as well acquainted as D. with "the secret motives of the expressions and omissions," or rather better. Now I never was dexterous myself at unravelling enigmas; but if I were to argue from the inflammable disposition of Mr. H.'s opponent, I should say his name might be deduced from the Latin verb *Uro*, to burn; probably it coincides with the imperative mood second person singular, *Ure*. Should this deduction be correct, the name very much resembles that of Dr. Ure, at Glasgow, of controversial notoriety. Let it be observed I do not positively "ascribe the papers" to Dr. Ure; but should he not be the author, "he may think it worth while publicly to disown them." On the contrary, should my solution of the enigma be correct, he will perhaps, before he again wield his potent pen against Mr. H., favour the world with a lecture on the best method of *calculating* latent heat*.

I could now, if I chose, enumerate some curious instances of C.'s adroitness at forgetting his own proposed imaginary experiments, groundless and unnecessary assertions, &c. which, when he found the tables turned against him, he has prudently and silently, but not very gloriously, abandoned. I could likewise expose some artful attempts to traduce and calumniate the character of Mr. Herapath as a scientific writer, which would make a singular contrast with the claims C. would lay to candour, "integrity," and "honourable feeling." But these things I choose to pass over.

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* See Mr. Herapath's correction (Annals for Dec. 1821, Note, page 458) of two unaccountable errors of principle Dr. Ure committed in computing the latent heat of water, from his own experiments in his paper, Philos. Transac. 1818, p. 388. Dr. Ure computes the latent heat to be 967° instead of 888°, which Mr. H. shows it should be from the Doctor's own experiment.

Few people dislike anonymous controversy more than I do. Attacks of this kind have ever appeared to me in the light of character assassinations; and hence I would wish to see them banished from society. But when once such productions are allowed to appear—what is to be done? Ought we to permit them to pass unnoticed? Ought we to suffer their authors to triumph uncorrected? Or, rather, Is it not a duty we owe to ourselves and the community to expose the vanity of their pretensions? And if, with due regard to our own character, this cannot be done *in propria persona*, we must of course descend to their own level, and oppose them anonymously. This is the very method I have taken with C. He commenced an attack on Mr. Herapath in a style, to which no person who valued his credit could openly reply. Perceiving the shallowness of this writer, and that he sought to cover a want of depth and soundness by quaint terms and flippant expressions, I felt an inclination, as no other person appeared to take it up, to lay open his hollow pretensions in the way that I have. The consequence is, that, finding himself foiled, he, though a volunteer aggressor, has been the first to complain. Writhing under the justice of my remarks, and convinced of the badness of his cause, he flies out into open invective against my occasionally using, in a contest which no one advised him to commence, the identical weapons of his own choice. Can the world desire a greater or more decisive proof than this of his own conviction of the unshaken stability of his adversary's case? He threatens indeed to palliate his discomfiture with "contempt;" an excellent shield, surely, for an inglorious defeat. Without descending to an employment of the term, the world can perceive how much above it my sentiments of him rise. However, I by no means wish to discourage his resolution. I think it the wisest course, under present circumstances, he can take. Should no credit flow from such a retreat, there will at least result this comfort to myself and advantage to him,—that I shall avoid further trouble, and he further exposure.

I am, gentlemen, yours, &c.

D.

LV. *On a new Sextant recently invented and constructed by Professor* Amici *.

Modena, July 3, 1822.

When in September 1820 you did me the honour to visit me in my laboratory, amongst many instruments which I had the pleasure to show you, your attention was particularly fixed upon a combination of two glass prisms, by the motion of which the angular distance of two distant objects might be measured. In the construction of this little model, my intention was merely to give geographers and sailors a small commodious instrument and one easily rectified for the measurement of angles from 0 to 180 deg. within two or three minutes of the truth: but the sketch of this instrument so much pleased you, and you perceived that such great advantages might be gained by the use of it, that you encouraged me to pursue the idea, and to give greater perfection to it. Since that time I have, as I promised you, occupied myself about it; and having completed this instrument, I have now the honour to transmit you a description of it.

If we place a plane mirror before the object-glass of a telescope, we easily perceive that two objects distant from one another may be seen at the same time in the field of the telescope, one by direct rays, the other by those that are reflected. But if we desire to see the same object directly, and at the same time by reflection, it is well known that the thing is impossible, because the rays are no longer reflected when they are parallel to the reflecting plane.

It follows from this, that if at the object end of a telescope a moveable plane mirror be placed upon a graduated circle, it cannot serve to measure angles, on account of the impossibility of determining the collimation; that is to say, the beginning of the division of this graduated arc. But if, instead of a mirror, we make use (as has been my practice) of an isosceles and rectangular glass prism, besides the obtaining a greater quantity of reflected light, we may also observe the coincidence of two images of the same object, one produced by direct rays, the other by reflected ones, and thus determine by this method the ZERO point in the division of the instrument.

Let, then, A B C, Plate IV. fig. 1, be a prism placed before the object-glass E, so that its greatest face B A be in a line with the axis of the telescope directed towards the distant object Q, the parallel rays which come from one point of the object Q, falling upon the side B C of the prism, will be returned by refraction towards the plane B A, and after having undergone a total reflection will be again reflected upon the side A C, and go out parallel to their first direction: all these rays passing through the object-glass will form at its focus the image reflected from the point Q, which will fall exactly upon the direct image formed in this same focus by parallel rays which come from the uncovered half of the object-glass, by means of which the point zero of the graduated limb may be ascertained. Nevertheless if we turn the prism round its edge A, on the side B C A, it will present new objects in succession coinciding with the object Q, until the side A C shall be parallel to the object-glass; we shall then have the super-position of all those points that are 90 degrees distant from the point Q. It is therefore clear, that by this means we can measure all angular distances as far as 90 deg. *and a little more:* I say *a little more*, because that depends on the extent of the refraction of the glass*; and if the prism be still turned on the same side, the total reflection of the rays upon the plane B A will no longer take place.

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* In a Letter from the Professor to Baron Zach, contained in the *Correspondance Astronomique*, vol. vi. page 551.

If, before the other half of the object-glass, which we have supposed not to be covered, a second prism, equal to the first, be placed, but moveable in a contrary direction, the two objects will then be equally seen by reflection; and by the combined movement of the two prisms we can carry the measure of an angle to the double of the greatest angle measured by one prism alone.

These principles, upon which rest all the theory of angular measures by these prisms, being well understood, it will be easy to comprehend the construction and the use of this new instrument. Fig. 2 shows it in perspective, as it was traced in a *camera lucida.*

A B D is a sector greater than a quarter of a circle of four inches radius. The limb is divided from 10 to 10 minutes, and by means of the vernier we can read to 10 seconds. About the centre C turns the index C E, which carries the vernier at one extremity, and the isosceles rectangular prism F at the other, with its edge S C directed towards the centre and perpendicular to the plane of the limb. The other prism H, equal to the first, is fixed in its place on the instrument, and so disposed that when the index marks *zero*, the larger faces of two prisms are nearly in contact and perfectly parallel. Lastly; a telescope N, supported by the arm L I, is moveable on the plane of the sector about the centre C. M is a microscope by which to read the divisions: and this forms the whole of the instrument.

The effect of this double motion of the telescope parallel to the limb, is, that the object-glass can receive a greater quantity of rays from one prism than from the other; so that we may by this means render the images of two objects, which differ in brightness, equally luminous; similar to what is obtained by Hadley's reflecting sextant, by the elevation or depression of the telescope upon the limb. But if by this motion we do not obtain this equality of light, on account of a too great difference of brightness in the two objects, we apply to the object-glass of the telescope the cover A (fig. 3.), half of the circular opening of which remains uncovered, whilst the other half is covered with a plane coloured glass. This glass, being turned towards that prism which reflects the image that is too luminous, will temper its too great brilliancy: and, as the cover may be turned any way, this coloured glass may always be placed before that part of the object-glass upon which the prism throws too much light: thus, when we make the observation with the instrument reversed, we can cause that image to be reflected from the prism the brightest, which had before been the least bright: and the mean of these two observations will then be exempt from the error of the parallelism of the planes of this coloured glass, if such a defect exists.

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* With a prism of common glass angles as far as 102 degrees may be measured.

We may detect by this instrument the error of collimation in three different ways.

First. We may effect this by the coincidence or super-position of two images of the same object; the one direct, the other reflected. The sun's disk is to be preferred; but any terrestrial object whatever will answer this purpose, provided it be not nearer than 50 toises; for it is only at this distance that the parallax of the instrument begins to be imperceptible.

Secondly. By the coincidence of two images of the same object, exteriorly reflected from two small sides of the prisms. In this case we shall have an angle of 90 degrees. Indeed the two isosceles and rectangular prisms having their great sides parallel, when the vernier marks *zero*, will have turned 90 degrees, when the two smaller sides become parallel.

Thirdly. By measuring two angular distances of two objects almost diametrically opposed. The excess or defect of the sum of these two angles upon 180 degrees will be equal to the moiety of the angle to add to or deduct from the *zero* point given by the vernier, in order to have the true *zero*, or the error of collimation:—for example; Let the angles of two objects be diametrically opposed, the one of 85 and the other 97 degrees, the true *zero*, or the real beginning of the limb's division, will be $85^\circ+97^\circ=182^\circ-180^\circ=\frac{2}{2}^\circ=1^\circ$.

In comparing this last verification with that obtained by the first method, if there be a difference, it will be an error in the division of the limb. This, however, supposes that the prisms be made with the utmost precision, and that the axis of the telescope shall be always perpendicular to the common section of the reflecting sides of the prisms.

With respect to the telescope, it is easy to show, that if its axis be inclined towards the common section of the reflecting planes of the prisms, the fourth part of the true angle will have for its sine, the sine of the fourth part of the angle given by the instrument, multiplied by the co-sine of the inclination of the axis.

In effect, let (fig. 4) S R, S T, be the two reflecting planes intersected by S Q; let us suppose a plane S V, which divides this angle, that we draw the right line A B perpendicular to S Q, and the oblique line A D = A B = 1. From D draw the perpendicular D H upon B A, and from the points D and B let fall the two perpendiculars D E, B C upon the plane S B; lastly, draw the right lines E A, C A, and H F parallel to C B. Now it is known by the principles of optics, that if B A represent the axis of the telescope, the angle formed by two coincident objects by reflection is quadruple the angle C A B, or twice the motion of the index. But if the angle has the obliquity D A, the true angle is the quadruple of the angle D A E, although the index gives the same angle as it had marked before.

To show the error, then, it is sufficient to determine the value of the angle D A E, by means of the known angles C A B and B A D; by the construction we have A H : H E : : A B : B C, or because H F = D E cos D A H : sin D A E : : 1 : sin C A B; from whence we shall, as I have said, have sin D A E = sin C A B cos D A H.

By this formula we perceive that the greatest error must take place when C A B = 45°. In this case, if the axis of the telescope has only one degree of inclination, the angle observed, instead of 180°, will be only 179° 57′ 56″: but this error, produced by a defective position of the telescope, is reduced to nothing, if we attend to the making our observation in that part of the field of the telescope where the slightest contact of the objects takes place.

I shall not speak here of those errors which may result from imperfections of the prisms, since these may easily be calculated, or readily corrected by the artist. Neither shall I say whether the instrument constructed by me, as a first essay, has answered the attempt. You, Sir, who have proved it, will be the best judge of it. All that I can say is this: that after having by the use of it ascertained its defects, and the corrections of which it is susceptible, I flatter myself that in executing it upon a larger scale I shall bring it to a degree of perfection that shall leave nothing to be desired.

Note by Baron Zach.

All those who know by theory and practice the reflecting octant or sextant of Hadley, and who will compare it with the reflecting sector of M. Amici*, will without doubt perceive the innumerable advantages which this last instrument has over the former. The octant of Hadley is founded upon this simple principle in catoptrics,—that if rays of light diverging or converging are reflected by a plane mirror, they diverge or converge after reflection towards another point on the opposite side of this surface, and to the same distance as the first point. It follows that if the rays of light coming from any point of an object be successively reflected by two plane mirrors, a third plane perpendicular to the two mirrors, crossing the point of emission, will also cross the two images successively reflected. All the three points will be at an equal distance from the common intersection of the points of the real object, and of its image formed by the second reflection; they will make an angle double of that of the inclination of the two mirrors.

All the instruments of reflection are constructed upon this principle, and accordingly are furnished with two mirrors: a third is sometimes added, to make what is called the *back observation:* these three mirrors have from thence taken the names of the great moveable mirror, and of small fore and back mirror: the last two being fixed upon the instrument.

Many scientific men have endeavoured to render perfect this instrument, intended chiefly for the navy. Grant, Ewing, Dollond, Magellan, Ramsden, T. Mayer, Borda, and others, have modified and transformed it in various ways; but the principle has always continued the same,—the reflection of mirrors placed at different distances, upon the plane of these instruments.

The sector of M. Amici has no mirrors; its reflections and refractions are made by two isosceles and rectangular glass prisms, in the way in which the inventor of this instrument has explained in his description.

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* M. Amici has given to his instrument the name of Reflecting Sector: the reflecting Octants or Sextants are instruments of an arc of 45° or 60°; this of M. Amici is of more than 100°; thus it is neither octant nor sextant nor quadrant: it is a Sector. It is true, that in Astronomy an instrument whose arc contains but a few degrees, is denominated a Sector; but in Geometry, this classification exists not: besides, the two kinds of instruments will always be distinguished, the one being called a Zenith Sector, the other a Reflecting Sector.

It results directly from this construction, that the images reflected from the surface of these prisms lose not so much of their light as if they were reflected from mirrors. In Hadley's octant, one image is always more weak than the other; in the sector of Amici, we may temper, modify and equalize the light at pleasure, according to the motion given to the telescope N, moveable upon the two pivots L and I (fig. 1) of the arm that supports it, and according to the rotatory motion given to the cap A with its coloured half-glass placed (as he has described) before the object-glass.

In the second place, in the sector of Amici, the instrument has no parallax in the angles taken with objects very near to the observer; because, from the construction of the instrument the two prisms which give the two images are always quite near to each other. If there be any parallax, it is so insignificant that it rarely merits consideration: nevertheless, if we are fearful of it, we have only to determine the error of collimation by the same objects of which we would take the angles, and apply it to the angle indicated upon the limb of the instrument. It is different in Hadley's octant, where the two mirrors are placed at some distance from each other. The octant marks upon the limb that angle which a ray from the object reflected from the centre of the great mirror makes with a ray from the other object from the centre of the small mirror, or, what comes to the same thing, from the observer's eye; it follows that when the first of these lines cuts the other precisely at the focus of the telescope, the octant then marks the exact angle; but if the intersection of these two lines falls any where else, there will be a difference between the angle indicated upon the limb, and that which the objects subtend, as appears to the eye: we must then apply a correction to all those angles made from very near objects. The treatises upon these instruments teach the process for finding these corrections. This advantage in M. Amici's sector, of being exempt from making any sensible parallax, will especially be appreciated by navigators, who in making surveys of sea-coasts are often obliged to make use of objects sometimes very near and sometimes at a great distance, to mark down the soundings on their charts.

In the third place, the crystal prisms of the sector of Amici are not subject, like the mirrors, to flaws or cracks, and sometimes to the destruction of the silver.

To avoid this inconvenience, mirrors of platina have been proposed, and actually made: but this metal not being susceptible of a fine polish, the old mode has been resumed.

The glass mirrors are sometimes liable to be bent or warped by the pressure of the adjusting screws, which are placed behind the frame upon which they are mounted. Messrs. Ludlam and Dollond have in a most ingenious manner remedied this; but there have been instances where mirrors substantially mounted in brass frames, by the sole effect of the expansion of that metal from the sun's heat, have been more or less warped, which is so much the more dangerous as the observer has no means of being aware of it. Nothing of this nature is to be feared from the manner in which the prisms are mounted on M. Amici's sector; they are never subject to be impaired, and are exposed to no error; they may always be kept clean and neat, are easily fixed and carried, one upon the sector, the other upon the index; they are never cramped in their frames or in their motions.

In the fourth place, the greatest and most essential of all the advantages which the instrument of M. Amici has over that of Hadley, is that we can make the *back observation*, that is to say, with the horizon of the sea, diametrically opposite, without adding a third prism; because, as we have seen in the description given by the inventor, we can measure with this sector an angle of more than 180 deg. This advantage is really of the first importance and utility not only for seamen, who coast along shores, which intercept from their view the horizon of the sea on that side on which the sun is, but also for astronomers and geographers, who would use this instrument upon land by the help of an artificial horizon. The difficulties attending the rectification of mirrors for the back observation are well known, which occasions their being so rarely used, and that they are even suppressed upon all sextants*.

The astronomers, who with this instrument and the artificial horizon would determine upon land the latitude of places by the double meridian altitudes of the sun, cannot, particularly in low latitudes and for a long time together, use the sextant. The greater part of these instruments can measure only to 124 deg., at least those of Troughton and Schmalkalder go only to that limit. Thus at Genoa, from the 7th of May to the 8th of August, we cannot with these instruments take the double meridian altitudes of the sun. At Cairo, no observation can be made from the 26th March to the 17th September; and under the equator they can never be used for this purpose. It is on this account that M. Ducom in his *Course of Nautical Observations*, &c. Bourdeaux 1820, page 94, in making his remarks on the method of determining latitudes by *non-meridian* altitudes, says, "This application may be of indispensable utility when we want to find the latitude of a place, and where the meridian altitudes are too great for an artificial horizon."

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* On this head may be consulted a very important memoir contained in the 74th vol. of the Gentleman's Magazine, and in the 14th vol. of the Naval Chronicle for 1805, page 21; the title of which is "A demonstrable, accurate, and at all times practical method of adjusting Hadley's sextant, so as to render the back observation equally correct with the fore observation; and to measure an angle of 150, 160 or 170 deg. as accurately as one of 30, 40 or 50 deg., communicated to the Astronomer Royal by letter dated 28th September, 1803, by the Rev. Michael Ward of Tamworth, Staffordshire."

This is not the case with respect to the sector of Amici; with this instrument we may take the double meridian altitudes of the sun under the tropics and even at the zenith. When M. Amici came to Genoa in May to bring us his reflecting sector, we could no longer at that time take in the artificial horizon meridian altitudes of the sun with any of our sextants, which, however, M. Amici was able to do with his sextant. On the 17th and 18th of May he took thirteen circum-meridian altitudes of the sun; we shall here mention only those made at noon. The 17th of May the altitude of the superior limb of the sun was = 64° 56′ 20″; the 18th of May, = 64° 38′ 10″, error of collimation + 10′ 50″. These observations, with a number of others since made towards the summer solstice, have always given us, within a few seconds, the latitude marked in our little observatory.

We could here enumerate many other advantages; as for example, that we can verify upon this sector the point zero, and the point 90 deg., as has been seen in the description: That in taking altitudes on an artificial horizon, the telescope always rests in a horizontal position, so that the observer may sit at his ease before it, without changing the position either of his body or head, whatever be the altitude of the star he is observing: That at sea we may take all the altitudes of two opposite horizons, the anterior and posterior, the mean of which will correct the *mirage* and all other irregularities of the refraction: but on these subjects we shall attain more knowledge and experience when M. Amici has finished a large 12-inch sector of reflection, which he has in hand at present, and to which he will adapt all those improvements which the use and practice of this instrument have suggested to him, during the stay which this ingenious and much-esteemed philosopher made with us.

The small 4-inch sector, which M. Amici has above described, and which is the first that was ever made, is now in our possession, and we habitually use it in taking correspondent altitudes of the sun. We are both proud and jealous of being possessed of the first model of so fine an invention; which, like all other useful novelties, will require time to make way through, and get rid of, ancient modes and usages, which through interest and prejudice are so difficult to be discarded. But as all truths require time to subdue the obstinacy of habit and prejudice, a little patience will conduct us to this issue. It required half a century before Hadley's most excellent invention had established that empire which afterwards became universal.

When in 1819 we recommended in the 2d vol. of the *Correspondance*, page 387, to our great opticians, to apply their talents to the perfection of reflecting instruments, we were far from hoping that our wishes would be accomplished so soon: a great step has now been gained, and let us hope that others will be made; for the proverb says, *Inventis facile est addere.* To prove the truth of this adage, we shall begin by proposing here a small addition. The coloured glass applied to the cover of the object-glass (fig. 3), can be inverted, but not brought back again. To correct its defect of parallelism, if there should be any, we only have to add on the other side of the cover a little piece of tube such as is seen at A, and then we can apply this cover with its half-glass coloured in the two sides before the object-glass, and by this means return this glass on the same part of the object-glass, either for its verification, or to correct or remove the error which will occur in the observation, if the sides of this glass be neither plane nor parallel. Before we finish this Note, we cannot refrain from making some remark upon the liberal and straightforward manner in which M. Amici has published his new invention here. Many others would have solicited prizes, rewards, patents, or at the least that it should receive the honourable mention or favourable report of some academy, or of some board of longitude. M. Amici despises all these practices, the springs of which are so well understood at this time; he prefers to publish with frankness and clearness such ideas of his as may be of public utility. We shall finish this note with a quotation from the letter of one of our most celebrated astronomers, whom we had made acquainted with M. Amici's new instrument, and who replied to us in the terms following:

"Truly the discovery is important, and I share completely in your admiration. An invention like this, made in England, would have enriched its author*. If there were any means of ensuring to M. Amici a just reward, it ought to be thought upon; for such is the simplicity of this instrument, that, its description once given to the public, it may every where be executed. M. Amici's is so much the more worthy of praise, as he seeks not his own advantage by his invention, which he readily gives to the public. What reward was given to Hadley I know not; but .... will not offer us the opportunity to search into this accompt, and to gratify ourselves with the comparison."

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* It is pretended that the caleidoscope produced this effect for its inventor: this instrument nevertheless is not of such utility as the sector of reflection; yet it is more amusing, and therefore, &c. &c. . . .

[We believe that the surmises of these learned foreigners respecting the riches acquired by inventors in England are altogether erroneous; and that those inventions by which the public are so much benefited, are too frequently attended with great loss to their projectors and those who engage in bringing them to perfection.—EDITORS.]

LVI. *Observations on the occasional Appearance of Water in the Cavities of regular Crystals; and on the porous Nature of Quartz, and other crystalline Substances, as the probable Cause of that Occurrence.* By JOHN DEUCHAR, *M.R.A.I. Edin., M. Cal. Hort. Soc., M. Wernerian Soc., O.P.A. Royal Phys. Soc., and Lecturer on Chemistry in Edinburgh.**

AMONG the numerous arguments which have been brought forward by those who support the aqueous formation of the globe, no one seems to bring with it greater force than that which is drawn from the fact that water is often found inclosed within the cavities of crystals of a regular shape. The ready conversion of water into steam, and the expansive nature of the steam when formed, even at a moderate temperature, would lead us to conclude that crystals containing confined globules of that fluid, could never have an igneous origin. When we examine the nature of such crystals, we often find them composed of very infusible materials, such as quartz, the fluate of lime, &c. The intense heat requisite to bring quartz to the state of fusion which would be necessary to enable it to cool into regular crystals, must, if not counteracted by an immense external pressure, have boiled the contained water, and formed an elastic agent so powerful that the texture of no known substance could resist it: but, as we cannot, when we examine the situations in which such crystals are found, discover the slightest proof of any such external counteracting pressure, we are naturally led to conclude that the whole must be the result of some other mode of formation; and that theory which ascribes it to crystallization from solution in water, seems satisfactory enough.

Those who maintain this latter opinion have generally contented themselves with explaining the circumstance upon the supposition, that the particles of the substance in solution, during

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* Read before the Wernerian Natural History Society, on Saturday May 19, 1821, and communicated by the Author.

ring the process of concreting into the crystalline form, had surrounded a portion of the fluid in which it had been dissolved. Now, however satisfactory this may appear in accounting for some specimens, it must be allowed that there are others again with regard to which it entirely fails.

A theory may be proposed, more consistent with the appearances of artificial crystallization, and quite authorized by a comparison of the same substances as produced by the manufacturer, and as found crystallized in nature. Newly formed crystals contain an excess of water in their constitution: this excess, provided the substance be not of a deliquescent nature, must gradually lessen, till the neutral state, as we may call it, be acquired. The excess of water can only escape from the interior of a crystal by capillary motion; and should there, by any malformation of the nucleus or any part of the interior, be left a void space in the crystal, then the capillary attraction may be induced internally as well as to the surface.

When we examine natural crystals, we find them possessed of a peculiar compactness of cohesion, and tardiness of solution, which we do not find with regard to those artificially formed. The natural sulphate of lime appears inert to water; and the natural carbonate of baryta appears equally inert to the strong muriatic acid: but the artificial salts are readily acted upon, the former by the water, and the latter by the acid. Not only is this found to be the case, but we observe also that the artificial crystals gradually acquire from age a greater or less degree of the inertness of natural ones. The water of crystallization, therefore, in artificial salts must be in a great excess; but were they exposed for a long-enough time under similar circumstances, we need not doubt that they would acquire the compact and inert character of natural ones. The large crystals of sulphate of magnesia, when first formed at the manufactory, contain more water in their constitution than when kept some time; and if so, how does the water escape? It must be through the pores of the salt; and the same may be applied to every artificial crystal. Now, if we go back to the origin of natural salts, the strong presumption of their porous nature will appear. Whether we admit or deny the general formation of minerals from an aqueous solution, we must at least grant that many of the crystals found in nature have been formed through the agency of water; and if this be the case, we have no reason to believe that Nature would, at the early period of the formation of these compact crystals, resort to any other mode than she at present adopts in completing the formations commenced by the manufacturer: we cannot but suppose that these natural crystals, like those of art, had at first a similar excess of water of crystallization, which through a series of ages had escaped by capillary attraction through the minute pores of the mineral.

It was whilst attending Professor Jameson's lectures on natural history, in 1810, that I first directed my attention to these curious specimens; and having often heard various accounts given of empty bottles well corked, which had been sunk in the ocean and brought up full of water, for which I could only account by granting the porous nature of glass, I began to suspect that all siliceous bodies might be porous. As the experiments with empty bottles were in general not properly attested, and were often contradictory, it became necessary to have them repeated with care and minuteness: several friends sailing for different parts of the East and West Indies, were ready to promise me the requisite information, which was to be tried upon thick glass globes about five or six inches diameter hermetically sealed at the glass-house as soon as made, from which circumstance each presented a partial vacuum; (see figure 1; *a b c d* are the notches by which it was attached;)

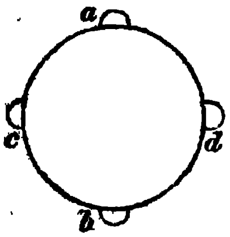

fig. 1.

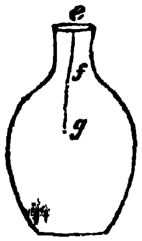

fig. 2.

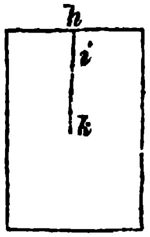

fig. 3.

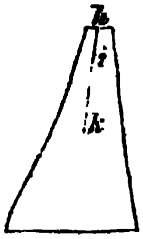

fig. 4.

but unfortunately the promised experiments were either mismanaged, or forgotten in the hurry of commercial concerns.

In 1813 my attention was recalled to the subject by the following appearance, which occurred during some experiments with boiling water. Upon inadvertently pouring hot water into a crystal decanter, which had a crack at the mouth about three inches in length, I observed it dart downwards to nearly five inches from the top; when I ceased pouring in the hot water, the crack ascended again to the third inch; the application was repeated many times, and presented the alternate extension and contraction of the crack each time as at first. (See fig. 2; *e f* shows the crack, and the dotted line *f g* shows the extended crack which closed up again.) Upon applying pressure to the top of pieces of window glass, laid flat upon a table, I found small cracks at the edges also alternately advance and retire. (See figs. 3 and 4; *h i* is the original crack, *i k* the extension of it; the pressure was applied at *k*; when the crack was in the pointed part of a fragment of glass, such as fig. 4, it was found to succeed best.) Upon communicating these results, several years afterwards, to Mr. Sivright, of Meggetland, he informed me, that upon laying by for a few days pieces of glass in which he had formed cracks of considerable extent, by means of a hot iron, he had often found them to disappear again.

From the above circumstance, the conclusion was obvious, That water might enter the void interstices of crystals, when aided by pressure, not only from the porous nature of their particles, but also from their temporary display of rents during the application of a high temperature. Such an explanation as this might be equally applied to either theory for the formation of our globe.

Recalled again to my former inquiry, by the above facts, I renewed my endeavours to get the hints regarding the porous nature of glass, if possible, established from the results of bottles or globes sunk to various depths in the sea. At the same time it is to be kept in view, that although the success of these experiments would bear strongly in favour of the doctrines which have been laid down, yet their failure by no means militates against the mode of crystallization proposed.

I shall now, therefore, lay before the Society a few experiments, for which I am indebted to Mr. John Grant. I give the account as communicated to me:

"Sir,—When becalmed and nothing stirring, it was a frequent amusement of the officers to let a bottle, either empty or full of fresh water, down to a considerable depth in the sea, 100, 150, or 200 fathoms, with a heavy weight attached.

"The result obtained on various occasions they found to be uniformly the same.

(1.) "When the bottle was let down empty, and the cork not properly secured, though it was driven in as hard as a cork could be, and the upper or outer part projecting considerably over the neck of the bottle; it was forced in and the bottle was full of salt-water. It made no sort of difference whether the bottle was sent down *bottom* or *top undermost*; nor was it to be expected it would, as the pressure on hauling up must have been as much as on letting it down.

(2.) "If the cork was properly secured with sealing-wax, and a number of folds of oil-cloth, and all covered with painted canvass well fixed upon the bottle,—the cork came up *in statu quo*, but the empty bottle was now full of salt-water.

(3.) "If the bottle was sent down with the cork properly secured as before, and full of fresh-water, it invariably came up *with the cork exactly as it went down*, but the bottle was full of *salt-water*. I never examined the water with sufficient accuracy to be able to say whether it was of the precise saltness of the water in which it had been immersed; but from my personal observation I am certain it was not by any means the water which was sent down; the water brought up was turbid, smelt strongly, and had a nauseous saline taste like the sea-water.

"On various occasions, upon examining the cork, it was found that *the water had not even penetrated to it*, so well had it been secured; and upon drawing it, and *cutting it across, it was found perfectly dry*.

"The bottles used were porter ones; they were generally kept down for a short time, not exceeding a quarter of an hour.

"These experiments were performed on board the Minerva frigate. I was twice on deck during the whole of the process of the experiments: the first time was in June 1811, about the latitude of 40° North; the second time was in May 1812, near the northern tropic. I remain yours respectfully,

"*Mr. John Deuchar, &c.*" "JOHN GRANT.

I have laid before the Society, with the greater satisfaction, the account given by Mr. Grant, as I find it in most respects established by the facts since mentioned by Mr. Campbell in his account of his travels in South Africa, and still more recently by those introduced by Mr. Perkins into his paper upon the compressibility of water.

[To be continued.]

LVII. *On a new circular Micrometer: communicated in a Letter from* M. LITTROW *to* F. BAILY, *Esq.*

"HAVE you heard of the *suspended circular micrometer* made by Frauenhofer at Munich? You are aware that this simple instrument is in general use on the continent, particularly amongst the German astronomers. If used in the manner I have described in my *Astronomie* (vol. i. page 358), it is certainly a *very useful* instrument: and M. Delambre is wrong in treating it so contemptuously in his work on astronomy. However, it has hitherto been difficult to obtain a perfect circle, which should be unconnected with the sides of the telescope: for, if it had been ever so well finished, it was easily bent by taking it off its support, and fixing it in the tube. And it may perhaps be doubted whether there has hitherto been one perfect micrometer, unless the diaphragm itself were used for that purpose: which latter, however, renders the observation of the *entrance* of a star very uncertain and troublesome."

"M. Frauenhofer conceived the fortunate idea of placing, in the focus of the telescope, a *glass* pierced in the centre with a hole sufficient to admit of a metal ring, which was attached thereto, and which ring was then turned perfectly circular: and, as it was never afterwards separated from the glass, its circular form remained uninjured."

"The great advantage attending this method is that the metal circle requires no other support, since the glass itself is fixed to the diaphragm: and thus the double concentric circle (which forms the micrometer) seems to hang without support in the expanse of the heavens."

[Our mathematical instrument-makers occasionally construct the micrometer of a transit instrument by means of fine lines drawn with a diamond on glass: and I conceive it would not be more difficult to draw *two concentric circles* in the same manner; which would probably answer every purpose to which the circular micrometer is usually applied. There appears to be only one objection to this plan; which is, that the loss of light (which however is not great) may prevent the observation of *small* comets, and *very minute* stars; to which class of objects the circular micrometer is peculiarly adapted.—F. B.]

LVIII. *On Pyroligneous Æther.* *By* Philip Taylor.

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—In the Philosophical Magazine for August last, I observed the following extract from one of the foreign journals*:

"On examining different samples of pyroligneous acid, M. Döbereiner lately found *alcohol* in two of them obtained from birch-wood."

As I believe there is an error in this statement, I shall trouble you at this time with a few remarks upon it, and in a future number of the Magazine detail more fully the experiments which have led me to the conclusions I have formed on this subject.

The peculiar fluid which M. Döbereiner calls alcohol was first discovered by me in the year 1812; at which time I was extensively engaged in the manufacture of pyroligneous acid and the various preparations formed with it. In attempting to purify this acid by a new process, I observed that both the colouring matter and that which gives it its characteristic odour were held in solution chiefly by a very volatile and inflammable fluid. Having separated some of this peculiar fluid from the acid, and deprived it of its colouring matter by repeated rectifications, I was struck with its strong resemblance to alcohol in many of its characters.

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* Schweigger's N. J$^{\text{l}}$. für Chem. u. Phys.

I found it perfectly miscible with water:—that it dissolved camphor and all the gum resins quite as readily and as abundantly as alcohol does; that it burned with a flame like that of alcohol; and that its specific gravity varied from 830 to 900, according to the care taken in its rectification.

As I had convinced myself that this spirit might be obtained in considerable quantity, and would be useful for various processes in the arts, I was naturally led to think how far its similarity to spirits of wine might subject it to the excise laws; and I made a series of experiments to ascertain if it could truly be called alcohol.

Many of these experiments were rather interesting; but I will at this time only mention one, which, in my opinion, proves its non-identity with alcohol.

Taking a quantity of this spirit which I had rectified as perfectly as possible, I added to it the same proportion of sulphuric acid that is usually employed to produce sulphuric æther. Instead of obtaining æther, I found a spirit still miscible with water, and burning with a blue flame; its smell being somewhat altered and its specific gravity a little reduced.

The residuum in the retort was a black pitchy substance, which became perfectly hard and brittle on cooling.

From what I have stated it is obvious that this fluid is neither alcohol, nor an essential oil, but probably a new variety of æther.

M. Döbereiner appears to suppose that birch-wood especially yields this spirit; but I have found it equally in pyroligneous acid from various other woods.

It appears to me that there is no difficulty in accounting for the formation of this spirit; which, from its greater resemblance to æther than to any other substance, I have called *pyroligneous æther*. It is well known that no acetic acid exists in wood; yet on exposing it to destructive distillation acetic acid is formed: that is to say, the carbon, hydrogen, and oxygen are liberated from their original combination by the action of the heat; and meeting together under favourable circumstances, they recombine and form acetic acid. The pyroligneous æther is also a ternary compound on the same three simple substances, but in different proportions; and the quantity of it produced will be found to vary according to the circumstances under which the distillation of the wood is conducted. I have constantly found in the decomposition of wood, coal, and oil, that both the proportions and qualities of the products are greatly influenced by the temperature, and construction of the apparatus. On decomposing coal, the proportions of the gases, as well as of the ammonia, tar, essential oil, naphthaline, &c. will greatly depend on these circumstances. On decomposing oil, the products may be diversified by the apparatus employed and by the mode of conducting the operation. I pointed out on a former occasion, that the decomposition of this substance commences at the temperature usually called its boiling point; and I believe the effect called ebullition in this case is merely produced by the extrication of gas; and if fixed oil is thus operated upon, a considerable portion of it is converted into volatile oil. At a higher temperature the carbon, hydrogen, and oxygen of the oil, will either form their full proportion of gas fit for illumination, or acetic acid will be produced, and gas of less illuminating power according to the conditions under which the operation may be conducted.

I have merely sent you these remarks, that the attention of other scientific men who have more leisure may be tempted to pursue a subject which appears to me an interesting one. And I shall be happy if they produce a little inquiry into the nature of the singular fluid which I have described.

I am, gentlemen, yours most truly,

Bromley, Middlesex, Oct. 18. PHILIP TAYLOR.

I have sent Mr. Garden, of 372 Oxford-street, some of the pyroligneous æther, that any gentleman wishing to examine it may know where to obtain it.

LIX. *On a Lunar Iris, or Rainbow by Moonlight.*

To the Editors of the Philosophical Magazine and Journal.

Gosport, October 22, 1822.

GENTLEMEN,—KNOWING that you are desirous of gratifying your readers with descriptions of rare meteoric and atmospheric *phænomena*, I inclose you one for the Philosophical Magazine and Journal, on the *Iris Lunaris* that appeared here last evening; with a short account of three more that have also been seen here during the last five years.

I am, gentlemen,

Your very obedient servant,

WILLIAM BURNEY.

Last evening at 51 minutes past 6 o'clock, the eastern limb of a lunar iris appeared immediately over Portsmouth Dockyard, and in two minutes afterwards the other limb appeared over Portsdown-hill, the iris as yet incomplete: at 55 minutes past six, the moon shining in a clear space, with an altitude of only 7½°, a distance of 29° westward of the meridian, and the rain moderately descending, our gratification was completed, at the expense of a wetting, by the appearance of a perfect *lunar iris*, of a silvery colour in a black passing *nimbus* or rain-fraught cloud to the N.E. No prismatic colours were distinguishable in any part of the iris, the moon not having come to her first quarter; consequently her light seemed too faint to produce a variety of colours in the bow.

The extent of the iris along the plane of the earth's surface was 81° 34′; that is, within 3° of its greatest extent, on the supposition of an observer standing on a plain ground, with the moon in the horizon: and the altitude of its apex above the horizon was upwards of 27°. By comparisons of this measurement with solar rainbows, when the sun has had a similar altitude, we conclude that there is no perceptible difference in the extent of the solar and lunar iris.

On the 19th December 1820, we observed a faint *lunar iris* to the westward, in a thick fog, from 7 till 8 P.M.; but the altitude of the moon being upwards of 30°, the apex of the iris was not more than 10° or 11° above the horizon.

On the 15th October 1820, we observed a perfect *lunar iris* to the N.E., with part of an exterior bow above it, during a shower at 9 P.M., in which some faint prismatic colours were discovered, the moon at that time being almost in the middle of her second quarter.

On the 25th of August 1817, we also observed a perfect *lunar iris* to the N.W. from 30 to 40 minutes past 8 P.M. on a large but slowly passing *nimbus*, the moon being to the S.E. and nearly full: prismatic colours were distinctly traced in this iris; but they certainly bore no near comparison with those of the solar rainbow that appeared the following afternoon.

In all these appearances of the lunar iris, the weather at the respective times was unsettled and stormy for several days together: the phænomenon, however, is not a prognostic, but rather the effect of a series of sudden storms.

From these remarks we may conclude, that the best time to look out for such a rare phænomenon is in stormy weather, when the *nimbi* pass in quick succession over us, and when the moon has a low altitude and is at least five or six days old (12 or 14 or 16 would be better): but the nearer she is to the horizon at either rising or setting, the more beautiful and extensive will the silvery arc appear with its delicate prismatic colours in the front of the rain-cloud, which must come from or near the quarter in which the moon appears.

☞ *Note on Mr.* Murray's *Paper on the Relation of Acids and Alkalies to vegetable Colours.*

In reference to Mr. Murray's letter which appeared in our last Number, p. 170, it has been suggested to us to refer our readers to vol. v. p. 125; vol. vi. p. 152; vol. xi. p. 402, of the Quarterly Journal of Science, where Mr. Faraday's *original* remarks on the action of boracic acid and other acids on turmeric paper are stated as they occurred to him some years since:—Also that it may perhaps occur to Mr. M. that, as he had not seen Mr. F.'s previous observations, Mr. F. probably had not seen his when he wrote the additional observation, vol. xiii. p. 315.

Page 171, line 17, *for* Turmeire *read* turmeric.

LX. *Notices respecting New Books.*

Elements of the Philosophy of Plants. By A. P. Decandolle, and K. Sprengell. Translated from the German.

Transactions of the Horticultural Society of London, vol. v. Part I. 4to. 1*l.* 11*s.* 6*d.*

A System of Mechanics. By the Rev. J. R. Robinson. 8vo.

Remarks on the present defective State of the Nautical Almanack. By Francis Baily, F.R.S. and L.S. 8vo. 2*s.* 6*d.*

Practical Electricity and Galvanism. By John Cuthbertson, 8vo. 12*s.*

Preparing for Publication.

In a few days will be published, in one neat pocket volume, a new Edition of Mr. Parkes's Rudiments of Chemistry, carefully corrected, and adapted to the present state of chemical science. This edition is printed on a larger and better paper than heretofore, and the new matter which has been added since the publication of the first impression, has enlarged the volume full one-fourth beyond its original size. A new and copious Index will be appended to the work, and it will be illustrated with several highly-finished Copper-plate Engravings of Chemical Apparatus.

ANALYSIS OF PERIODICAL WORKS ON ZOOLOGY AND BOTANY.

The Botanical Register. No. 92.

Two elegant and beautiful species of *Thysanotus* (Brown *Prod.*) commence this month's Number. The plants of this singular genus are all natives of New Holland; and, until very lately, have been strangers to our gardens. *Th. isantherus*, Pl. 655, is distinguished by bulbous roots; and *Th. junceus*, which follows, is likewise figured in this month's Number of Curtis's Magazine.

Pl. 657. *Elæocarpus reticulata.* 658. *Papaver bracteatum.* This plant has been already so well figured (only a few months back) in Mr. Lindley's elegant work, that we see no reason why it is repeated in this publication, particularly as Mr. Lindley's description is copied: moreover, it occupies the space of two plates, which could have been devoted, with much more advantage, to some other of the increasing number of unrecorded plants abounding in the London gardens.

Pl. 659. *Ancilema sinica*, a new plant from China, thus defined: A. caule ramoso diffuso; foliis ligulatis, acuminatis, racemulis alternis subsenis supernè in paniculà positis: staminibus tribus barbatis quorum uno castrato; sterilibus tribus nudis.

Pl. 660. *Passiflora pallida*, an interesting plant of this elegant genus.

Pl. 661, *Argyreia cuneata*, is a genus distinguished from *Convolvulus* and *Ipomœa*, " principally by an indehiscent seed-vessel with one-seeded cells."

Curtis's Botanical Magazine. No. 429.

To prevent a constant repetition of numbers, we shall in future notice the plates in their regular series, merely giving the number of the first plate, which is 2350, and represents *Arthropodium cirratum* Willd. beautifully figured on a double plate. *Thysanotus junceus*, the same plant, unluckily, as we have noticed above in the Register: from a comparison of the two plates, it appears this has been made from a dwarf specimen. The dissections, however, in this are an advantage the other does not possess.

Crinum aquaticum, a plant, brought from Southern Africa by Mr. Burchell, which Mr. Herbert has here described. Like *Amaryllis revoluta* and *insignis*, it is not, probably, a distinct species from *Am. ornata*, but Mr. H. is obliged, by the principles he has laid down in defining the genera of Amaryllideæ, to propose it as a genus possibly distinct from any other. Some botanists will consider this as an additional proof that he is wrong in what he has done in his proposed *reformation* of the Order; and will question whether his new species of *Crinum* and *Amaryllis* be any more distinct than the gardeners' varieties of Tulipa, Geranium, &c. Instead of a concise specific character (after the excellent example of Linnæus) we have a detailed description too long for us to copy.

Alstrœmeria pulchella, Linn., from China. *Passiflora lunata*; we strongly suspect the leaf represented in outline, as a variety, is, in fact, a very distinct species; as, from native specimens in our Herbarium, the flowers are stated to be very large and white. *Crinum arenarium;* given as another new addition to this genus, but we doubt whether it be specifically different from *Cr. asiaticum:* we must protest against the novel mode Mr. Herbert has here introduced of making descriptions and even specific characters of plants *from measurement;* because until all the plants of one species can be made to grow of one size, such measurements will only suit the individual sample from which they have been taken. Mr. Herbert is a botanist of ability, and we hope he will give this subject further consideration.

Swainson's Zoological Illustrations. No. 25.

This number commences the third volume of this beautifully executed work. Plate 120 represents *Ampullaria corrugata*, and the figures and descriptions sufficiently point it out as distinct from *A. globosa*, figured on the plate of the 2d volume; it is thus defined:—A. testâ globosâ, corrugatâ, olivaceâ; spiræ prominentes, acutæ, anfractibus ventricosis; aperturæ margine crasso, fulvo, sulcato; umbilico parvo, juxta labii interioris mediam posito; operculo testaceo. Mr. S. observes, that Mr. B. Sowerby has mistaken this shell for the *Amp. rugosa* of Lam., which has a thin, and not a thick and reflected, margin round the aperture.—Plate 121: This is the most beautiful plate yet given in the work, and represents a new and superbly coloured Creeper, belonging to the African genus Cinnyris, but erroneously placed by Dr. Horsfield among the Nectariniæ, which, Mr. S. remarks, are all American birds. *C. Javanica*, supra nitidè purpureo-ærata, subtus olivaceo-crocea; scapulis, uropygio, strigaque laterali a rostro ad pectus descendente nitidè violaceis; jugulo castaneo; cauda nigra.

Plates 122 and 123 contain four uncommon varieties of the true *A. virginea* (*Bulla virginea*, Lin.), under which name so many shells have been placed. Mr. Swainson's definition of the characters which belong to this variable species, is as follows:—*A. virginea*, testâ elongatâ, fasciis numerosis nigris viridibus et flavis ornatâ; anfractus basalis latitudine altitudinem superante; aperturâ rotundatâ labio exteriore integro; basi profundè emarginatâ.—*Licinia crisia*, Sw.: the male and female of this insect differ in a remarkable manner, and had not observations been made on the living insects, we should almost have doubted their connexion.

Greville's Scottish Cryptogamic Flora.

Mr. Greville's last No. (4) contains the curious *Echinella fasciculata*, which by many would be considered of animal origin; but which, as neglected by the zoologist, we are happy to see (whatever may be its real nature) taken up by the botanist: *Puccinia Buxi*: *Amanita nivalis*, a delicate new species which well merits the name, whether we consider the snowy whiteness of its hue, or its truly alpine situation among the snow of the highest of the Grampian Hills: *Uredo effusa* (the *U. Spirææ* of Sowerby): and a new species of *Næmaspora*, *N. Rosarum*. Under this last individual we have an observation, "that the Genus *Næmaspora* may be found eventually to border too closely upon many species of the old Genus *Sphæria*." The justness of this remark will be apparent to those who will trouble themselves to compare the excellent magnified section of the *Næm. Rosarum* with those of *Cryptosphæria Taxi*, (*Sphæria Taxi* of Sowerby,) figured in Mr. Greville's 3d Number. It is by such admirable analyses of the parts of fructification of these obscure plants, that we may expect much new light to be thrown upon the subject.

LXI. *Intelligence and Miscellaneous Articles.*

LAW OF CALORIC STATED BY M. LAPLACE.

In an inquiry on the attraction of spherical bodies and the repulsion of elastic fluids, M. Laplace unfolds in the *Annales de Chimie*, xviii. 185 *seq.*, the following remarkable law agreeing with experiment:

"The quantity of heat which is disengaged from a bulk of gas passing under a determined pressure from a higher into a lower temperature, is proportional to the square root of this pressure."

This accounts, among other things, for the great advantage of high pressure steam-engines; the pressure which the steam exerts being proportional to the caloric contained in a given space, which may be considered as an unity; the pressure or tension of the steam increases in a much greater ratio than the quantity of caloric, that is to say, fourfold, while the caloric is doubled.

DISTRIBUTION OF TEMPERATURE IN METALS BY MECHANICAL CONCUSSION.

It is well known that carters are in the habit of well hammering the axles of their waggons, before they put them in motion in a hard frost, in order by this means to guard the iron which has become brittle against breaking. The utility of this practice in great and sudden changes of temperature in a metal becomes obvious, if we recollect that in Laplace's and Lavoisier's experiments on the expansion of metallic rods, these rods would only take an uniform temperature in their whole length when they had been subjected to percussion. *Ann. de Chim.* xviii. 35.

OBSERVATIONS OF THE PRESENT COMET, BY PROFESSOR HARDING OF GŒTTINGEN.

Gœttingen, M. T.		·	Right ascension.	Declination.
		h. ′ ″	° ′ ″	° ′ ″
1822.	August 21	12 17 46	265 55 09	+53 44 45
	22	13 05 45	264 19 21	52 08 59
	24	11 40 25	261 34 02	48 58 55
	26	11 39 31	259 05 31	45 37 38
	27	12 56 53	257 55 24	43 47 24
	Sept. 2	11 09 13	252 43 35	33 25 16
	4	11 16 43	251 23 53	30 01 44
	14	11 20 34	246 44 10	14 04 26
	15	9 59 31	246 25 39	12 47 18
	16	7 40 47	246 08 46	11 29 11
	17	8 25 55	245 50 56	10 06 29
	25	7 59 23	244 02 49	+ 0 31 47
	28	7 59 11	243 58 16	− 3 28 17
	October 6	7 44 19	242 35 07	− 9 45 00
	10	7 03 47	242 14 31	−12 46 38

From observations of August 21, 27, and Sept. 2, Professor Harding derived the following parabolical elements:

Time of Perihelium, 1822, October 23,	2h. 45′ 1″
Long. of Perih. - - - - -	272° 28′ 31″
Long. of ☊ - - - - -	92 24 50
Inclination of orbit - - -	52 28 46
Log. of shortest distance - -	0·062358

Motion retrograde.

The comet was discovered by Pons at Marlia, July 13, and by Gambard at Marseilles, July 20. In Germany it was first found on the 20th August: when it was nearly visible by the naked eye. Mr. Enke at Seeberg has clearly convinced himself, by comparing earlier observations made at Marseilles with those of August and September, that its orbit is elliptical, and he has found the following elements.

Time of passage through the Perihelium	October 24·99374, Mean Time of Seeberg.
Long. of Perih. - - - - -	270°31′30″·7
——— ☊ - - - - - - -	93 04 52·4
Inclination - - - - - -	52 39 41 8
Log. of shortest distance - -	0·0545019
Eccentricity - - - - - -	0·96617805
Log. of half the great axis - -	1·5253033

Motion retrograde.

These elements agree with the arc described during two months to 0′5. Mr. Enke is now engaged in further correcting them by later observations, which, however, must soon be closed, as the comet will become invisible in the twilight.

ADIPOCIRE OF CORPSES.

The adipocire or fatwax of putrified carcases, which is quite different from cetine and cholesterine, consists, according to Mr. Chevreul (*Ann. de Chim.* xviii. 65), of margarine acid, sebacic acid, and an orange yellow principle, and is formed, according to experiments, by the action of the carbonate of ammonia, which is disengaged by putrefaction upon animal substances.

RETURN OF THE NORTH-WEST LAND EXPEDITION.

The late accounts of this Expedition which reached Great Britain through Montreal, prepared us, in some degree, to expect its return by the annual Hudson's Bay fleet. On Friday the 16th, Captain Franklin, Dr. Richardson, and other officers of the Land Expedition, arrived in Edinburgh, having posted from the north, where they were landed from the Hudson's Bay ships. Their discoveries, we understand, will entirely alter our present views of the geography of the northern regions of North America. We are also informed, that the Gentlemen of the Expedition one of whom (Doctor Richardson) is a native of this city, are of opinion that Capt. Parry will in all probability be able to double Icy Cape, and reach the South Sea.—*Edinburgh Observer.*

The Expedition was fitted out in the summer of 1819, and in the course of the following year was enabled, by a liberal aid and reinforcement from the North West Company, to advance to the shores of the Great Bear Lake, situated in about 66 degrees north latitude, where it encamped and wintered. In the ensuing spring it approached the Copper-Mine River, which it descended until it fell into the Ocean. Hitherto the Expedition was accompanied by Mr. Wintzel, a clerk to the North West Company, with ten Indian hunters; but the open sea, which appeared at the confluence of the river with the Ocean, elated the travellers so much with the hope of ultimate success, that it was thought proper to dispense with the further attendance of Mr. Wintzel and his hunters, who returned up the river, leaving the Expedition to proceed in two canoes to survey the coast of the Polar Sea, eastward from the mouth of the Copper-Mine River, towar[illegible]son's Bay. But in consequence of the approach of winter so early as the end of August, heavy falls of snow, dense mists, and an extremely bare wardrobe, the Expedition was prevented exploring further than about 500 miles of the coast to the north-east of the Copper-Mine River, and ascertaining that the sea before them was quite open and free of ice.

As the Expedition returned, its wants became alarming in the extreme, and it soon required the utmost fortitude and exertions to brave the hardships which presented themselves. In approaching that part of the Copper-Mine River from which it set out, it was necessary to double an immense point of land, which would occupy a great length of time; it was therefore deemed necessary to set the canoes adrift, and cut a direct course over land to the Copper-Mine River. They crossed, after much difficulty, in a canoe constructed with the skins of elks which they had killed; but in making their way to the Great Bear Lake, they fell completely short of provisions, and were for many days under the necessity of subsisting upon sea-weeds, and the powdered bones of the food they had already consumed. In this situation Mr. Hood, nine Canadians, and an Esquimau, fell victims; and had not the survivors exerted themselves by a super-human effort to reach the Great Bear Lake, it is probable they would have all perished. Here they found the heads and bones of the animals that served them for last winter's provisions, which preserved them until their arrival at some post belonging to the Hudson's Bay Company.

Farther Particulars.—Captain Franklin has succeeded in surveying the northern coast of North America, from the mouth of Copper Mine River, for more than 500 miles to the eastward. He found the mouth of that river in lat. 67 deg. 48 min. which is four deg. less than what Hearne made it, and no point of the coast to the eastward exceeded 68 deg. 20 min.: in one place it came down to 66 deg. 30 min. to the Arctic Circle. The sea was studded with innumerable islands, between which and the main land was an open channel of water four or five miles wide, and from ten to forty

fathoms

fathoms deep; no ice whatever, but some small masses here and there adhering to some rock or promontory, all of which is highly favourable to the success of Captain Parry, who, however, could not have arrived on the part of the coast to which Captain Franklin proceeded until the latter had left it on his return, which was on the 25th of August, and at which early period the winter set in, and continued with great severity, though, as every body will remember, we had no winter at all in England.

On the 5th of September, on their return by land, a snow storm occurred, which covered the earth with two feet deep of snow: this was the forerunner of all the misfortunes that befel the party. The musk oxen, the rein-deer, the buffaloes, and immense flights of birds, immediately hastened away to the southward. Their provisions were all expended, no firewood was to be had; the fatigue of dragging their baggage through the snow induced them to leave their canoes behind. With great difficulty, and in the utmost distress from cold and want of food, they reached the Copper Mine River, which lay between them and Fort Enterprise, where they had passed the previous winter, and where they expected to find a supply of provisions. There was no wood to construct a canoe, or even a raft, and eight days of the only fine weather during the whole season were lost in fruitless attempts to cross the river, which was at length effected by a sort of boat or basket of rushes, which, with the utmost difficulty and danger, carried over the party, one by one, filling every time with water.

From this moment the Canadians began to droop, and before they reached their destination, not less than eight of them perished from cold and hunger, the whole party having subsisted almost wholly on a species of lichen which grew on the rocks, and by gnawing pieces of their skin cloaks. With exactly the same hard fare, and sometimes without even that for two or three days together, the five Englishmen, Captain Franklin, Lieutenants Hood and Back, Dr. Richardson, and a seaman, supported themselves by their buoyant spirits, and did all they could to cheer up the desponding Canadian hunters, but in vain; they became insubordinate, refused even to go out in search of game or firewood, straggled away from the rest of the party, and frequently laid themselves down on the snow, indifferent as to what might befall them.

With the most anxious desire to preserve their lives, Dr. Richardson and Lieutenant Hood consented to remain behind to attend to three of these infatuated people, who were unable from weakness to proceed. Two of them died, and the remaining one, a good marksman, and more vigorous than any of the party, became so savage and ungovernable, that he refused to endeavour to shoot any thing towards their subsistence, or even to fetch a little firewood, which Dr. Richardson and the English sailor were obliged to do; and while this savage was left alone in the tent with Lieutenant Hood, the latter being indisposed and sitting over a little fire, he shot him with his musket through the head, and killed him on the spot. After this he became more violent than ever, his looks were wild, and he muttered threats that could not be mistaken; so that Dr. Richardson, for his own safety, and that of the sailor, who had been a most faithful companion, found it necessary to get rid of the monster*, by shooting him through the head. Thus, of twenty persons which composed the Expedition, ten have perished:—eight through cold, fatigue, and famine, and two by violent deaths; but the rest of the party, after almost unparalleled sufferings, have returned to their friends and their country. It must be highly gratifying to the Naval Officers, that in their absence they were not forgotten, but that each has received a step of promotion in the service. Lieutenant Hood was considered as an excellent Officer, and an accomplished young man, who, among other acquirements, was an admirable draughtsman.

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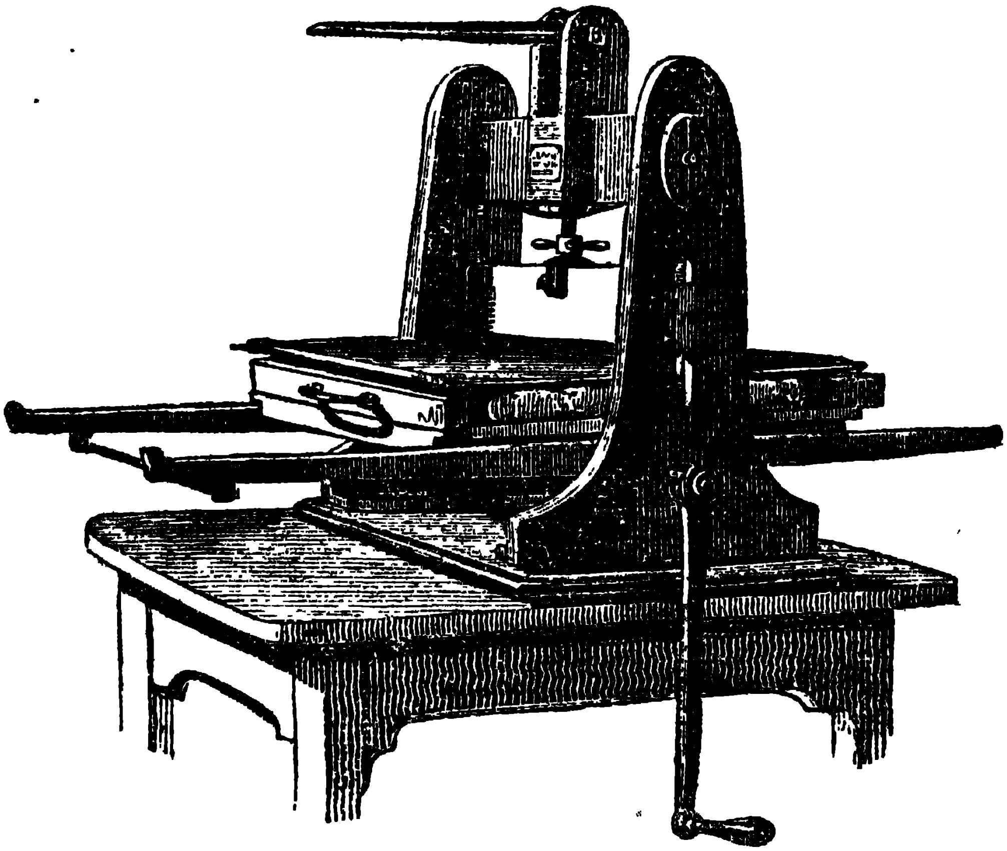
* The man was no doubt insane, in consequence of the hardships he, with the others, had gone through.

NEW LITHOGRAPHIC PRINTING PRESS.



The above Lithographic Printing Press has been invented by Messrs. Taylor and Martineau, engineers; and from the simplicity of its construction and consequent lowness of price, (being, we are informed, little more than half the cost of the machines hitherto in use,) it promises to extend the art of lithography both among amateurs and printers. We have ourselves examined this press, in consequence of the high opinion entertained of it by one of the first mechanics in this country; and as far as we are enabled to judge, we cannot but highly approve of the simplicity and efficacy of the machine. To those who are either amateurs or more deeply interested in the art, we recommend an inspection of it, at the Lithographic Establishment of Mr. Charles M. Willich, No. 8, Pickett-street, Strand, where it was shown to us, and every explanation afforded.

The pressure upon the surface of the stone is produced by depressing the lever in the centre, to which is attached an eccentric; and the motion to the carriage is given by the winch handle. There is a regulating screw above the scraper, by which the pressure is adjusted with the greatest accuracy. It may be necessary to inform such of our readers as are not acquainted with lithography, that the impression is given by the friction of the scraper, which is pressed on the stone while passing under it.

NAUTICAL ALMANAC FOR 1825.

This work has just made its appearance, but with scarcely any alteration from that of the preceding volumes, and without any assurance or prospect of improvement or enlargement for the future ones. We know not whether His Majesty's Ministers are asleep on this subject; but we trust that the House of Commons is not: and that a question of so much *public* importance will not be suffered to pass over another session without some *public* inquiry. The mode of conducting this work is a *public* concern: the money of the *public* is expended in bringing it to as great a degree of perfection as possible; and the *public* ought to enjoy any and every emolument which is derived from the sale of it. At all events, the proper officers ought to see that the whole of the profits, after making a liberal allowance to the bookseller, is appropriated to the improvement and perfection of the work. Now, we have some little experience in printing and publishing; and we cannot help thinking that a great deal *more* might be given for the *same* money. The case is very different from that of an individual author, who may expect, and *ought*, to be *repaid* for his time and labour. Here, the Government is the publisher, who neither desires nor expects to derive any emolument from it: no expense is incurred for advertisements: the sale of many thousand copies of the work is well ascertained and ensured: and, after making every allowance for the expense of *getting it up*, must secure a handsome profit to the publisher. For, even the *usual discount* to booksellers is not allowed on *this* work: *Sixpence* only on each copy is all that is deducted for *ready money*. So that the publisher secures to himself *Four Shillings and Sixpence* for every copy that is sold. But why should not a portion of this profit go towards the improvement and perfection of the work? Why should this country suffer itself to be eclipsed by the neighbouring nations, in this important part of its scientific fame, for the sake of enriching *one* individual? These are questions which we hope will attract attention in the proper quarter, and tend to remove the complaints which are now become pretty general on this subject. We shall probably advert to the contents of the work, in a subsequent number.

OBITUARY.

Lately died, on his way to Geneva, ALEX. MARCET, M.D. F.R.S. Honorary Professor of Chemistry at Geneva.

On the 25th October, in the 66th year of his age, after a lingering illness of nearly four months, died Mr. JAMES SOWERBY, F.L.S. M.G.S. &c., an artist of considerable talent, well known as the designer and engraver of the Plates, and the publisher, of the complete Flora of Great Britain, edited, under the title of "English Botany," by Sir J. E. Smith, the learned President of the Linnæan Society. He was a most intelligent and laborious cultivator of Natural History, as the numerous works in which his pen and pencil have been engaged amply demonstrate; and his memory will long be esteemed by those friends who had an opportunity of estimating his modesty, integrity, friendly disposition, and devotion to his favourite pursuits.

On the 13th died at Venice, after a short but severe illness, the Marquis CANOVA. He was universally celebrated as a sculptor of the first eminence, and revered for his worth as a man.

LIST OF PATENTS FOR NEW INVENTIONS.

To John Collier, of Compton-street, Brunswick-square, Middlesex, engineer, for certain improvements upon machines for shearing cloth.—Dated 27th September 1822.—2 months allowed to enrol specification.

To William Goodman, of Coventry, Warwickshire, hatter, for certain improvements in looms.—27th Sept.—6 months.

To John Bourdieu, of Lime-steet, London, esq. who, in consequence of a communication made to him by a certain foreigner residing abroad, is become possessed of a method or means of improving the preparation of colours for printing wove cloths.—27th Sept.—6 months.

To Benjamin Boothby, of the Iron-Works, Chesterfield, Derbyshire, ironmaster, for an improved method of manufacturing cannon shot, by which a superior shot is produced in the solidity and smoothness of its external surface.—27th Sept.—2 months.

To John Dowell Moxon, of Liverpool, Lancashire, merchant and ship-owner, and James Fraser, of King-street, Commercial Road, Middlesex, engineer, for certain improvements in ship cabooses or hearths, and also for apparatus to be occasionally connected therewith for the purposes of evaporating and condensing water.—27th Sept.—6 months.

To Frederick Louis Fatton, of New Bond-street, Middlesex, watch-maker, for certain improvements on or additions to watches or chronometers in general, whereby they may be rendered capable of marking or indicating the precise moment of any desired observations or rapid succession of observations, and without the necessity of stopping the regular movement of the watch as in ordinary stop-watches.—27th Sept.

To Thomas Timothy Beningfield, of High-street, Whitechapel, Middlesex, tobacco manufacturer, and Joshua Taylor Beale, of Christian-street, St. George's in the East, cabinet-maker, for certain improvements on steam-engines.—27th Sept.—6 months.

To John Whitcher, of Helmet-row, Old-street, St. Luke's, Middlesex, mechanic; Matthew Pickford, of Wood-street, London, common carrier and James Whitbourn, of Goswell-street, in the said county, coach-smit [illegible] for an improvement in the construction of the wheels of all wheele [illegible] riages, and all other vertical wheels of a certain size.—27th Sept.—2 [illegible]

To James Frost, of Finchley, Middlesex, for a method of casting [illegible] structing foundations, piers, walls, buildings, arches, columns, [illegible] mouldings, and other enrichments to buildings.—27th Sept.—2 months.

To Samuel Pratt, of Bond-street, Middlesex, trunk and camp equipage manufacturer, for certain improved straps or bands to be used for securing luggage upon chaises or coaches, or for securing property (generally) when placed in exposed situations.—27th Sept.—6 months.

To Thomas Binns and Jonas Binns, both of Tottenham Court-road, Middlesex, engineers, for certain improvements in propelling vessels, and in the construction of steam-engines and boilers applicable to propelling vessels, and other purposes.—18th Oct.—6 months.

To William Jones, of the parish of Bidwellty, Monmouth, engineer, for certain improvements in the manufacturing of iron.—18th Oct.—2 mon.

To Stephen Wilson, of Streatham, Surry, esq., for a new manufacture of worsted.—18th Oct.—2 months.

To Samuel Francis Somes, of Broad-street, Radcliffe, Middlesex, ship-owner, for an improvement in the construction of anchors.—18th Oct.—6 months.

To Uriah Lane the younger, of Lambsconduit-street, in the united parishes of St. Andrew Holborn and St. George the Martyr, Middlesex, straw-hat manufacturer, for an improvement in the platting of straw, and in manufacturing bonnets and other articles therefrom.—18th Oct.—2 months.

To John Williams, of Cornhill, London, stationer, for a method to prevent the frequent removal of the pavement and carriage paths for laying down and taking up pipes, and for other purposes, in streets, roads, and public ways.—18th Oct.—2 months.

To Joseph Brindley, of Frinsbury, near Rochester, Kent, ship-builder, for certain improvements in the construction and building of ships, boats, barges, and other vessels for navigation.—18th Oct.—6 months.

METEORO-

METEOROLOGICAL TABLE,

The *London* Observations by Mr. Cary, of the Strand.
The *Boston* Observations by Mr. Samuel Veall.

Days of Month. 1822.	Thermometer.				Height of the Barom. Inches.		Weather.		
	London.			Boston.					
	8 o'clock Morning.	Noon.	11 o'clock Night.		Lond.	Boston	London.	Boston.	
Sep. 27	46	57	49	55·5	30·24	29·95	Cloudy	Fine	
28	50	56	50	57	·21	30	Cloudy	Cloudy	
29	54	58	46	58	·04	29·80	Fair	Cloudy	Rain, A.M.
30	45	60	50	57·5	29·87	·55	Fair	Cloudy	
1	49	61	56	58·5	·74	·48	Fair	Fine	
2	55	64	59	61·5	·80	·55	Cloudy	Cloudy	
3	60	66	60	63·5	·76	·40	Showery	Rain	
[illegible]	60	65	60	62	·80	·40	Showery	Cloudy	
[illegible]	60	66	52	64	·71	·27	Fair	Cloudy	Thun.& Lightning *p.m.* with showers
[illegible]	50	60	50	57	·64	·20	Stormy	Cloudy	Rain, A.M.
[illegible]	54	60	54	52	·67	·22	Fair	Rain	Stormy, A.M.
[illegible]	56	58	50	58·5	·60	·20	Rain	Cloudy	
[illegible]	58	64	52	56	·70	·20	Showery	Rain	
[illegible]	50	59	47	58	·99	·50	Showery	Fine	Rain P.M.
[illegible]1	46	59	51	56·5	30·20	·80	Fair	Fine	
[illegible]	52	60	59	58·5	29·80	·55	Cloudy	Cloudy	Light. at night
[illegible]	58	60	52	60.5	·45	·20	Stormy	Rain	Storm at night.
[illegible]4	44	50	42	50	·99	·65	Fair	Cloudy	
15	40	47	46	50.5	·85	·50	Showery	Fine	
16	50	53	47	48·5	·35	·15	Rain	Rain	
17	46	48	47	47	·37	·20	Rain	Rain	
18	45	50	46	49·5	·76	·40	Fair	Fine	
19	50	55	52	51	·45	·10	Stormy	Rain	
20	55	57	56	57	·50	·15	Stormy	Rain	
21	50	56	50	56	·64	·22	Stormy	Fine	
22	45	56	46	53·5	·82	·45	Fair	Fine	
23	47	56	56	54	·65	·33	Cloudy	Cloudy	
24	55	60	56	59	·40	·10	Cloudy	Cloudy	
25	56	60	53	59·5	·58	·15	Fair	Fine	
26	51	58	52	57	·49	·15	Cloudy	Fine	

The quantity of Rain since the 26th September amounts to 5 In. 35.

From the *Am. de Chim.* for August, we learn that the meteor observed on the 6th of that month, at a quarter past eight, was seen also at Havre, Mans, Cherbourg, and Southampton.

Errata in p. 163, line 4, from bottom, for *wook* read *wootz.*
171, — 17, for *Turmeire* read *turmeric.*
196, last line, for 34 read 34°.

THE
PHILOSOPHICAL MAGAZINE
AND JOURNAL.

30th *NOVEMBER* 1822.

LXII. *On the Measurement of the Progress of an Eclipse of the Moon with a Sextant or Reflecting Circle.* By T. E. Bowdich.*

It is impossible to observe the beginning of an eclipse of the sun or moon on ship-board with precision; but by measuring the progress of either with a sextant, at intervals of five minutes, advantage may still be taken of these phænomena for the determination of the longitude.

This method offers the great advantage of multiplying the angles, and consequently of diminishing the errors by which the partial observations may be affected.

It was first proposed, for eclipses of the sun, by Wales, who thus observed that of 1774; King, who accompanied Captain Cook in 1777, also availed himself of it; but in both instances the mere observations are recorded, without calculation, formula, or result.

Köhler appears to have been the first who recommended, and Humboldt the first who put in practice, the application of this method to eclipses of the moon; the latter thus determined the longitude of Ibague, within one-fifth of a degree: but as Oltmans, who calculated this observation, has merely given us the result without the formula, and as I do not know of any formula being in print, I thought it might be useful as well as interesting to submit the following, which is general, until a neater one may be discovered.

Let Δ = longitude ☽ − long. ⊙ − 180°
λ = latitude of ☽
$\Delta_{,}$ = augmentation of the relative longitude of ☽
$\lambda_{,}$ = movement in latitude of ☽
d = demi-diameter of ⊙
d' = ... do. . . . ☽
p = parallax of ⊙
p' = . . do. . . ☽
e = enlightened part of ☽ in minutes

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* Communicated by P. Barlow, Esq., Royal Military Academy.

t = the time before or after the instant of calculating the longitude of ☽

T = time for which the above elements (from the Naut. Almanac) are calculated

T′ = mean time of the observation.

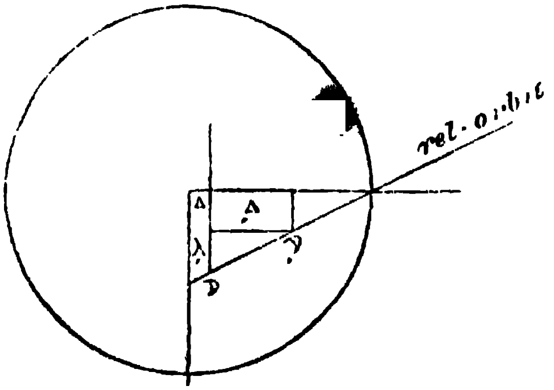
To determine the relative orbit of the ☽ we have

$$x = ay + b \quad x = \Delta \quad y = \lambda \quad \Delta + a\lambda = b$$
$$x = \Delta' \quad y = \lambda' \quad \Delta' + a\lambda' = b$$

by which latter equations we may determine a and b; but we need not recur to these equations, observing that when

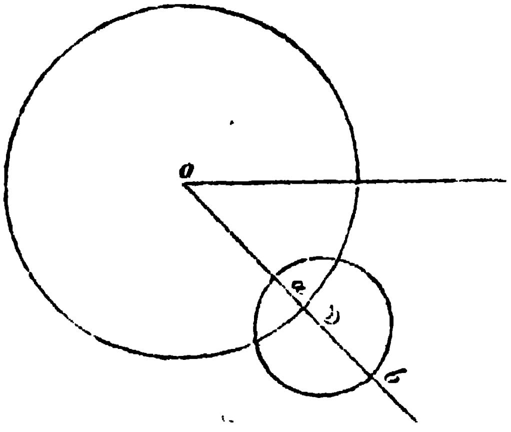
$t = 0 \quad x = \Delta \quad y = \lambda$
$t = 1 \quad x = \Delta + \Delta' \quad y = \lambda + \lambda'$
$t = \frac{1}{2} \quad x = \Delta + \frac{1}{2}\Delta' \quad y = \lambda + \frac{1}{2}\lambda'$

whence, the general expression $x = \Delta + t\Delta' \quad y = \lambda + t\lambda'$, which gives us the value of x and y in time, and enables us to determine the place of the ☽ at each instant.



To determine the distance of the centre of the moon and cone of umbra at any moment, we have (calling the radius of the cone of umbra ϱ)

$$D^2 = x^2 + y^2 = (\Delta_{,}^{2} + \lambda_{,}^{2})\, t^2 + 2(\Delta\Delta_{,} + \lambda\lambda_{,})\, t + \Delta^2 + \lambda^2$$
$$0\,\text{☽} = D \quad D + \text{☽}\, b - \varrho = e \quad D^2 = (\varepsilon + \varrho + d')^2$$
$$(\varepsilon + \varrho - d')^2 = (\Delta_{,}^{2} + \lambda_{,})\, t^2 + 2(\Delta\Delta_{,} + \lambda\lambda_{,})\, t + \Delta^2 + \lambda^2$$



To find the value of ϱ we have

$$ST = \frac{TT'}{\text{tang}\, p} \qquad SS' = \frac{TT'\,\text{tang}\, d}{\text{tang}\, p} \qquad TR = \frac{TT'}{\text{tang}\, p'}$$

$$JT : JR :: TT' : RR' \qquad JS : JT :: SS' : TT' \qquad JS - JT : JT :: SS' - TT' : TT'$$

$$\frac{TT'}{\text{tang}\, p} : JT :: \frac{TT'(\text{tang}\, d - \text{tang}\, p)}{\text{tang}\, p} : TT' \qquad JT = \frac{TT'}{\text{tang}\, d - \text{tang}\, p}.$$

JR =

$$\mathrm{JR} = \mathrm{JT} - \mathrm{TR} = \frac{\mathrm{TT'}}{\text{tang } d - \text{tang } p} \qquad \frac{\mathrm{TT'}}{\text{tang } p'}$$

$$\mathrm{JR} = \frac{(\text{tang } p' - \text{tang } d + \text{tang } p)\ \mathrm{TT'}}{\text{tang } p'\ (\text{tang } d - \text{tang } p)} \qquad \frac{\mathrm{TT'}}{\text{tang } d - \text{tang } p}\ .$$

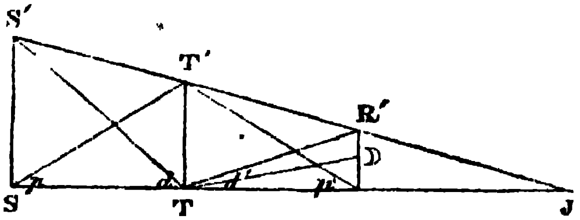
$$\frac{(\text{tang } p - \text{tang } d + \text{tang } p')\ \mathrm{TT}}{\text{tang } p\ \ (\text{tang } d - \text{tang } p)} :: \mathrm{TT'} : \mathrm{RR'}$$

$$\mathrm{RR'} = \frac{\mathrm{TT'}\ (\text{tang } p - \text{tang } d + \text{tang } p\,)}{\text{tang } p'} \qquad \text{tang } \varrho = \frac{\mathrm{RR'}}{\mathrm{TR}} = \text{tang } p -$$

$$\text{tang } d + \text{tang } p' \ \text{tang } \varrho = \text{tang } p + \text{tang } p' - \text{tang } d$$

$$\varrho = p + p' - d \mathrel{.\dot{}.} (\Delta_{,}^{2} + \lambda_{,}^{2})\, t^{2} + 2(\Delta\Delta_{,} + \lambda\lambda_{,})\, t + \Delta^{2} + \lambda^{2} = \gamma^{2} =$$

$$(\varepsilon - d' + p + p' - d)^{2}\ t^{2} + \ 2\ \frac{(\Delta\Delta_{,} + \lambda\lambda_{,})}{\Delta_{,}^{2} + \lambda^{2}_{,}}\ \ t = \frac{(\iota - d' + p + p' - d)^{2} - \Delta^{2} - \lambda^{2}}{\Delta_{,}^{2} + \lambda_{,}^{2}}$$



and making $\tau = (p + p' - d) - d'$ * we have

$$t = \frac{-\Delta\Delta_{,} - \lambda\lambda' \pm \sqrt{(\Delta\Delta_{,} + \lambda\lambda_{,})^{2} + (\Delta_{,}^{2} + \lambda_{,}^{2})[(\tau + \iota)^{2} - \Delta^{2} - \lambda^{2}]}}{\Delta_{,}^{2} + \lambda_{,}^{2}}$$

Thus this Problem serves to determine the longitude, when we have observed ε and the relative time at the place; for designating this time by T′, that of the calculation of the elements by T (which must be taken near the opposition) we have the longitude expressed in time by the formula $l = t - \mathrm{T}$, being west if l is positive, and east if it be negative.

The discussion of this formula shows, that if we make the quantity under the radical = 0, *i. e.*

$$(\Delta\Delta_{,} + \lambda\lambda_{,})^{2} + (\Delta_{,}^{2} + \lambda_{,}^{2})[(\tau + \varepsilon)^{2} - \Delta^{2} - \lambda^{2}] = 0$$

and derive the value of $(r + \varepsilon)$ therefrom

$$\tau + \varepsilon = \sqrt{\Delta^{2} + \lambda^{2} - \frac{(\Delta\Delta_{,} + \lambda\lambda_{,})^{2}}{\Delta_{,}^{2} + \lambda_{,}^{2}}}$$

it will be the value of the shortest distance between the centres of the *conus umbræ* and the ☽.

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* Or rather $\tau = \frac{61}{60}(p + p' - d) - d'$ "Pour faire ces calculs de demidurée, on fait ordinairement le rayon de l'ombre $\frac{61}{60}(\varpi + \pi - \delta)$, parce qu'on a remarqué que les durées observées étaient toujours plus longues que les durées calculées, ce qu'on attribue à l'atmosphère de la terre, qui intercepte la lumière du soleil, et fait le même effet que produirait une augmentation d'environ $\frac{1}{60}$ dans le rayon de la terre. Cette evaluation parait bien considerable."—Delambre, *Astronomie*, 350.

The time will be given by the equation

$$t = \frac{-\Delta\Delta_{,}-\lambda\lambda_{,}}{\Delta_{,}^{2}+\lambda_{,}^{2}} + \mathrm{T}$$

in which t indicates the middle of the eclipse.

If we would have the beginning and the end of the eclipse, we make $\varepsilon = 2d'$, and the time of each will be given by the equations

$$t = \frac{-\Delta\Delta_{,}-\lambda\lambda_{,} \pm \sqrt{(\Delta\Delta_{,}+\lambda\lambda_{,})^{2}+(\Delta_{,}^{2}+\lambda^{2})[(\tau+2d')^{2}-\Delta^{2}-\lambda^{2}]}}{\Delta_{,}^{2}+\lambda_{,}^{2}} + \mathrm{T}$$

taking the sign − for the beginning and + for the end.

The duration of the eclipse will be given by

$$\tfrac{1}{2}\,\mathrm{D} = \frac{\sqrt{(\Delta\Delta_{,}+\lambda\lambda_{,})^{2}+(\Delta_{,}^{2}+\lambda_{,}^{2})[(\tau+2d')^{2}-\Delta^{2}-\lambda^{2}]}}{\Delta_{,}^{2}+\lambda_{,}^{2}}.$$

If we desire the end of the immersion and the beginning of the emersion, we make $\varepsilon = o$, and the times will be given by the equation

$$t = \frac{-\Delta\Delta_{,}-\lambda\lambda_{,} \pm \sqrt{(\Delta\Delta_{,}+\lambda\lambda_{,})^{2}+(\Delta_{,}^{2}+\lambda_{,}^{2})(\tau^{2}-\Delta^{2}-\lambda^{2})}}{\Delta_{,}^{2}+\lambda_{,}^{2}} + \mathrm{T}$$

being after the middle of the eclipse if we take the sign + of the radical and before if we take −.

Let I represent the duration of the total immersion of the ☽, *i. e.* the time during which she remains completely invisible, and we have

$$\tfrac{1}{2}\,\mathrm{I} = \frac{\sqrt{(\Delta\Delta_{,}+\lambda\lambda_{,})^{2}+(\Delta_{,}^{2}+\lambda^{2})(\tau^{2}-\Delta^{2}-\lambda^{2})}}{\Delta_{,}+\lambda_{,}^{2}}$$

Lastly: we have the hour to which any enlightened part ε corresponds, by making ε equal to this part, and deducting the corresponding value of t.

In all these equations we use or repeat nearly the same logarithms, which very much expedites the calculation.

Let us suppose that we have measured the chord of distance between the two horns of the moon, which seems to me to admit of more precision; we have only to make the following additions in the original expressions for the elements,

$c = \frac{1}{2}$ distance of the horns of ☽

$t' =$ mean time of the observation of c.

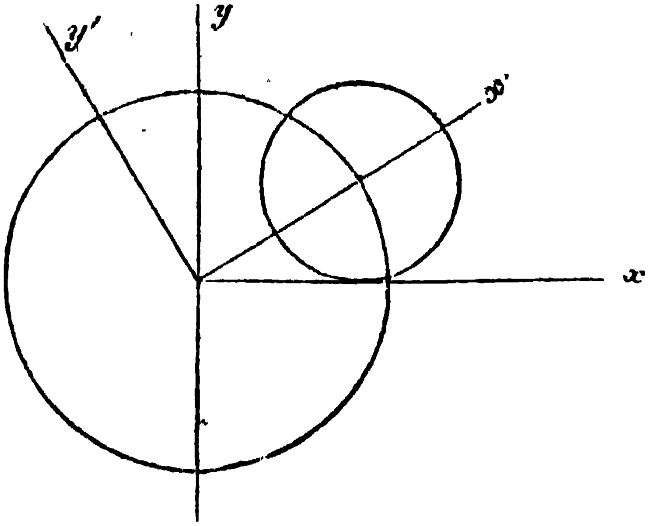
$\tau = \frac{61}{60}(p+p'-d) =$ radius of the section of the *conus umbræ.*

$\alpha = \Delta_{,}t+\Delta \quad \beta = \lambda_{,}t+\lambda \quad y = ax \quad a = \frac{y}{x} = \frac{\beta}{\alpha}$

tang $i = a$ sin $i = \beta$ cos $i = \alpha$

$y = \beta x' + \alpha y' \quad x = \alpha x' - \beta y'$ (*Biot, Geom. Anal. No.* 77.)

$x'^2+y'^2=r^2$ equ. of circle of *conus umbræ*. $y'^2+(x'-D)^2=d'^2$ equ. of circumf. of ☽ refl. to 2d axes.



$$\tau^2-x'^2+x'^2-2Dx'+D^2=d'^2 \quad x'=\frac{D^2+r^2-d'^2}{2D}$$

$$x'^2=\frac{D^4+\tau^4+d'^4+2D^2\tau^2-2D^2d'^2-2\tau^2d'^2}{4D^2}$$

$$y'^2=\tau^2-x'^2=\frac{-D^4-\tau^4-d'^4+2D^2\tau^2+2D^2d'^2+2\tau^2d'^2}{4D^2}$$

making $y=c$ we have

$$4D^2c^2-D^4-2D\tau^2-2D^2d'^2=-\tau^4-d'^4+2\tau^2d'^2$$

$$D^4+2D^2(2c^2-\tau^2-d'^2)=-\tau^4-d'^4+2\tau^2d'^2$$

$$D^2=-(2c^2-\tau^2-d'^2)\pm\sqrt{4c^4+r^4+d'^2-4c^2\tau^2-4c^2d'^2+4d'^2\tau^2}$$

$$D^2=-(c+\tau)(c-\tau)-(c-d')(c+d')\pm 2\sqrt{c^4-c^2\tau^2-c^2d'^2+d'^2\tau^2}$$

$$c^4-c^2\tau^2-c^2d'^2+d'^2\tau^2=c^2(c^2-d'^2)-\tau^2(c^2+d'^2)=(c^2-\tau^2)(c^2-d'^2)$$
$$=(\tau+c)(\tau-c)(d'+c)(d'-c)$$

$$D^2=(\tau+c)(\tau-c)+(d'+c)(d'-c)\pm\sqrt{(\tau+c)(\tau-c)(d'+c)(d'-c)}$$

$$A'=(\tau+c)(\tau-c),\ B'=(d'+c)(d'-c),\ D^2=A'+B'\pm 2\sqrt{A'B'}$$

$$D^2=\alpha^2+\beta^2=(\Delta_{,}^2+\lambda_{,}^2)t^2+2(\Delta\Delta_{,}+\lambda\lambda_{,})t+\Delta^2+\lambda^2$$

whence (making $-\Delta\Delta_{,}-\lambda\lambda_{,}=A \quad \Delta_{,}^2+\lambda_{,}^2=B$

$$t^2-\frac{2At}{B}=\frac{D^2-\Delta^2-\lambda^2}{B}$$

$$t=\frac{A}{B}\pm\sqrt{\frac{A^2}{B^2}+\frac{D^2-A^2-\lambda^2}{B}}$$

The time of the middle of the eclipse is $=T+\frac{A}{B}$

The four roots of the following equation

$$t=T+\frac{A}{B}+\sqrt{\left(\frac{A}{B}\right)^2+\frac{\tau^2+d'^2-\Delta\ -\lambda^2\pm 2\tau d'}{B}}$$

$t=$

$$t = \mathrm{T} + \frac{\mathrm{A}}{\mathrm{B}} \pm \sqrt{\left(\frac{\mathrm{A}}{\mathrm{B}}\right)^2 + \frac{(\tau \pm d'^{2}) - \Delta^2 - \lambda^2}{\mathrm{B}}}, \quad (c \text{ being } = 0)$$

gives the beginning of the eclipse,
the end of the immersion,
the beginning of the emersion,
the end of the eclipse.

The duration of the eclipse is $= 2\sqrt{\left(\frac{\mathrm{A}}{\mathrm{B}}\right)^2 + \frac{(\tau \pm d')^2 - \Delta^2 - \lambda^2}{\mathrm{B}}}$

The shortest distance between the centre of the moon and of the section of the *conus umbræ* (occurring when t is equal to the time of the middle of the eclipse, or $t = \mathrm{T} + \frac{\mathrm{A}}{\mathrm{B}}$) will be given by the value of D derived from the equa[illegible]

$$\left(\frac{\mathrm{A}}{\mathrm{B}}\right)^2 + \frac{\mathrm{D}^2 - \Delta^2 - \lambda^2}{\mathrm{B}} = 0 \qquad \mathrm{D} = \sqrt{\Delta^2 + \lambda^2 - \frac{\text{[illegible]}}{\mathrm{B}}}$$

Lastly: any enlightened quantity of the moon or any distance of the horns will be given by the formula

$$t = \mathrm{T} + \frac{\mathrm{A}}{\mathrm{B}} \pm \sqrt{\left(\frac{\mathrm{A}}{\mathrm{B}}\right)^2 + \frac{\mathrm{D}^2 - \Delta - \lambda^2}{\mathrm{B}}}$$

observing that in the former case $\mathrm{D} = \tau - d' + \varepsilon$.

in the latter, $\mathrm{D}^2 = \mathrm{A}' + \mathrm{B}' \pm 2\sqrt{\mathrm{A}'\mathrm{B}'}$

$$\mathrm{A}' = (\tau + c)(\tau - c) \qquad \mathrm{B}' = (d' + c)(d' - c)$$

Thus we may have the time t expressed in function of c or in function of ε. The longitude of the place will be expressed in time by the formula $\mathrm{L} = t - t'$, in which t' represents the time of the observations of ε or c; the longitude being east or west according as L is positive or negative. Finally; substituting the value of t, we have the longitude expressed in the time by the formula

$$\text{Long.} = \mathrm{T} - t' + \left(\frac{\mathrm{A}}{\mathrm{B}}\right) \pm \sqrt{\left(\frac{\mathrm{A}}{\mathrm{B}}\right)^2 + \frac{\mathrm{D}^2 - \Delta^2 - \lambda^2}{\mathrm{B}}}.$$

I have calculated M. de Humboldt's observation at Ibague* by this formula, and the result would no doubt accord precisely with that in the text, were the elements it contains free from errors; for, after correcting the most palpable, my result differs but 27″ from that of M. Oltmans. The following errors cannot be disputed, and other lesser ones certainly exist.

It is impossible that 21ʰ 20′ 45″ at Paris, can be the mean time, since the elements are calculated very near the opposition, and this happened, according to the text, at 19° 26′ 41″. If we suppose for a moment that this latter element is inexact, we may still convince ourselves that 21ʰ 20′ 45″ cannot be the correct time, by merely observing that the enlightened part of the moon 23′ 30″ must be near the end of the eclipse, since both the time and the quantity of the enlightened part of the moon continue augmenting; nevertheless, the time of this measurement is $21^h\ 0'\ 13'',9$, *i. e.* less than $21^h\ 20'\ 45''$, which is evidently absurd. This error of the text leaves no other alternative than to deduct the time, for which the observation is calculated, from that of the full moon, which gives us $19^h\ 27'\ 28'',8$. Another evident error of the text is detected as follows:

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* *Voyage de Humboldt, Astronomique*, (2 vols. 4to.) vol. ii. p. 255.

Mean time at Paris	21^h	$0'$	$13'',9$	according to the text.
Ibague	15	50	54 ,9	
Longit[illegible] in time	5	9	19	
instead of	5	9	39	given in the text.
Error			20″	

LXIII. *Reply to Captain* FORMAN *on the Theory of the Tides.*

By Mr. HENRY RUSSELL.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,— As I happen to have a particular veneration for old theories when I consider new ones incapable of exploding them, I shall, with your permission, reply to Captain Forman's communication of last month, in which he so warmly advocates the expansive theory of the tides.

Surely it does not follow, because we admit the expansion of water, that therefore we must, without consideration, relinquish our former theories of the tides! No. Let us call for the assistance of expansion when we can no longer do without it, and reject our present opinions when we discover their retention unnecessary, taking care, as far as we are able, to admit no more causes than are necessarily required to produce the effects.

As Captain Forman has proposed a box of marbles for my instruction, I shall propose for him a better way of trying the experiment. Fill the box at one end with wheat and the other with bran; of course we must consider the wheat the ebb, and the bran the flow: on agitating the box, the wheat will descend, and consequently the bran will be elevated.

Captain Forman next mentions an immense syphon, the existence of which of course I shall not argue; but, those who are acquainted with hydrostatics, well know that all liquids in open vessels act upon the principle of the syphon inverted.

The next point to which Captain Forman alludes, where he says, "I can prove in hundreds of instances that the tides are rising when his supposed cause has no existence," is decidedly hostile to his (Captain Forman's) hypothesis; for, if my cause has no existence, I would ask Captain Forman where is his cause? Now I wish to impress Captain Forman's mind with one particular fact, namely, that the instant his cause is removed, the effect ceases; whereas in the old theory, the waters having been removed, and consequently elevated, in endeavouring to recover their equilibrium, produce those inland flows which in many parts of the world do not arrive until the next tide, which is to give birth to the succeedi[illegible]flow, is even past its highest. Generally speaking, the age o[illegible]e tide when it arrives at London Bridge amounts to about twelve hours. The next point in question is the Atlantic Ocean; and here I confess it would be difficult to make 90 degrees out of only 60 or 70; but as it is well known to every experienced navigator that the Atlantic receives its principal supply from the Southern Ocean, I do not perceive that there is any need of conjuration.

The paragraph of the handful of water which Captain Forman has copied, I am sorry to say has undergone such a change as not only to convince me that he did not understand my meaning, but it has altered the sense materially; instead of "the moon has no power," it should be "the moon has not power;" again, "[illegible]magnet has no power," should be "a magnet has not power." I say a magnet has not power to lift a scale-beam, and yet it has power to disturb its equilibrium. And this I hesitate not to declare a perfectly analogous case with the equilibrium of the ocean, or with the equilibrium of any fluid in any open vessel whatever. If Captain Forman still thinks that it makes any difference whether the balance is supported from above or below, we may accommodate him with a simple contrivance.

Let a stick of about an inch in diameter be loaded at each end with a steel ball, taking care that its specific gravity be less than that of water: while swimming on its surface, by applying a magnet, the ball to which the influence of the magnet is directed will be sensibly elevated, although the magnet has not sufficient power to support the ball in the atmosphere. In this experiment the ball under the influence of the magnet is elevated by the superior gravity of the other ball, on the same principle that the flood tide is elevated by the superior gravity of the ebb; for take away the ball that is not influenced by the magnet, and the other immediately sinks; take away the ebb, and the flow immediately subsides.

As Captain Forman has manifested some uneasiness at my not further noticing his hypothesis, I shall select, as an extraordinary specimen of "philosophical arguing," that part of his hypothesis where he says, "We have only to take a handful of water out of the ocean, at the time of the rising of the tides, to be convinced that the expansion of the water is the immediate cause of the phænomenon; because, if the waters were pulled upwards by the power of the moon's attraction, and not pushed upwards by the expansion of the particles below, this water would not fall back to the earth, until the influence of the moon's attraction had gone off." This is, indeed, a mode of arguing which ought to be considered "unique in the annals of philosophy." Here is Captain Forman endeavouring to convince "every astronomer and every philosopher," that because the moon is not capable of sustaining a handful of water in the atmosphere, it has no power of elevating it whatever; and yet in another place he attempts to inform them how much the power of the moon is capable of diminishing its specific gravity.

Captain Forman gives it as his opinion, that the attractive power of the moon is to that of the earth as one to five hundred: now granting that to be the case, which let us admit for argument sake, I say 500 ounces of water in the absence of the moon would in her presence weigh only 499. It is the same with a handful of water; the moon attracts it in one direction with a power equal to 1, and the earth attracts it in another direction with a power equal to 500; the difference of these attractive powers at the same time, philosophers agree in calling the water's gravity; and to suspend it in the atmosphere requires that the attractive power of the moon should be equal to the attractive power of the earth. I am surprised that Captain Forman, with all his precision and accuracy, should not be able to discover a difference between the whole and a part.

The ease with which Captain Forman accounts for the tide on the opposite side to the moon, is very remarkable. He simply imagines a repelling power, which I defy him to prove the existence of, and the work is done.

If Captain Forman's imaginary repulsion should by his order be brought into existence, I promise him we shall very soon have no moon to argue about; as in that case (similar tides being elevated on opposite sides) attraction and repulsion must be equal, and therefore amount to nothing; the moon would have no centre to revolve about, and consequently would fly off in a tangent. The tide on the opposite side to the moon is occasioned by the centrifugal force which necessarily arises from the revolution of the earth and moon round their common centre of gravity, on the same principle that the waters at the equator are elevated from the centre by the diurnal revolution of the earth on its axis.

Captain Forman has made assertions in his postscript to which, of course, I cannot assent. I shall, however, notice a peculiar sort of convenience in which the Captain so freely indulges; namely, that of deciding without considering. He says, "Mr. Russell, I suppose, is so much of a philosopher as to know that whenever it is low water in any place, the tides are rising on one side of it and ebbing on th[illegible]ther; and if the 'superior gravity' of the water in this place cannot prevent the waters from ebbing on one side, it is not very philosophical to suppose that it can lift the waters on the other." Captain Forman would have saved himself the trouble of this last assertion, if he had previously considered that the earth is continually turning on its axis; and consequently is in regular succession exposing every degree of the equator on one side to the centrifugal force, and on the other side to the attractive power of the moon; and as the most dense waters (the ebbs) necessarily form an isosceles triangle with the moon, so are they ever ready to buoy up the flows.

I now take leave of this controversy, and promise in some future number of your Magazine to lay before your readers some observations on the expansibility of water as connected with the rising of the ocean. Henry Russell.

LXIV. *Observations on the Flexure of Astronomical Instruments. By Mr.* Thomas Tredgold, *Civil Engineer*[*].

The further improvement of the accuracy of astronomical observations being of great importance to the advancement of that science, a few remarks on the flexure of the parts of instruments may perhaps be regarded with interest by some of your readers.

A slight acquaintance with the properties of natural bodies must have informed even a careless observer, that no kind of matter is endowed with perfect inflexibility; and on a closer examination we find that every change in the position or points of support of a body, is accompanied by a corresponding change in its structure: the different parts of the body become extended or compressed in a different degree, or that which was extended in the first position becomes compressed by the change, and the reverse. In many cases these changes may be made sensible to the eye, and their effects may be shown in all bodies, when their magnitude is sufficient for applying proper instruments.

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[*] Communicated by the Author.

From these principles, then, which experience furnishes, we may anticipate, that a change of form takes place in every new position of an instrument; and consequently that it should be so constructed as to render the effect of these changes of form insensible in making observations; or, where that cannot be accomplished, the effect of flexure should be estimated, and the observations corrected accordingly.

In the construction of instruments, it is therefore desirable to inquire what form is adapted for any given purpose; and, assuming that the flexure will have a sensible effect, it will be of some advantage that the form be of the simplest kind, at least as far as regards the calculation of its flexure.

And here we may remark, that œconomy of material is of little if any importance, and therefore the forms of equal strength should be abandoned, and those of the least flexure substituted in their place. And since easy movement is not so essential as accurate movement, that form of axis seems the best which unites the greatest accuracy of form with that which is least flexible.

If an instrument be supported by a horizontal axis, and it be put in motion, the centre will rotate in a circle of which the radius is equal to the flexure of the axis at the point intersected by the plane of the graduated arc or circle.

Let us suppose the axis to be an uniform solid cylinder of which the radius is r, and the length from centre of pressure to centre of pressure l, W the weight of the moveable part of the instrument, and M the weight of the modulus of elasticity of the substance to a base of unity; also let p be the circumference of a circle of which the radius is 1 or unity; then

$$\frac{Wl^3}{12pMr^4} = \delta \text{ or the deflexion in the middle*.} \quad \text{(A).}$$

If the axis be a tube or hollow cylinder of uniform diameter, and r be the radius of the exterior part of the axis, and nr be the radius of the hollow part; then

$$\frac{Wl^3}{12pMr^4(1-n^4)} = \delta\text{, or the deflexion in the middle†.} \quad \text{(B).}$$

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* Practical Essay on Cast-Iron, &c. artt. 81, 87, and 100. The expression $\frac{f}{\epsilon}$ = M in this notation. The same equation may be derived from artt. 326 and 339 B of Dr. Young's Natural Philosophy, vol. ii.

† Pract. Essay on Iron, artt. 83, 87, and 100.

If the axis be diminished from the point where the weight acts towards the points of support, so as to be of equal strength in every part, the figure of each semi-axis will be that generated by the revolution of a cubic parabola. In this case the deflexion is greater; being, when $r =$ the greatest diameter,

$$\frac{3\,W\,l^3}{20\,p\,M\,r^4} = \delta^*. \quad \text{(C.)}$$

The same may be shown in the case of a hollow cylinder, and it may be proved generally that an uniform cylinder, whether solid or hollow, is stiffer than any figure inscribed within it.

When an axis is formed of cylinders of different diameters, the deflexion of the middle part will be $\frac{W(L^3-l^3)}{12\,p\,M\,R^4}$, and the deflexion of the extremities $\frac{W\,l^3}{12\,p\,M\,r^4}$; consequently the whole deflexion will be $\frac{W}{12p} \times \left(\frac{L^3-l^3}{M\,R^4} + \frac{l^3}{M\,r^4}\right) = \delta$. (D.)

Where L is the whole length, l the sum of the lengths of the small cylinders, R the radius of the middle, and r the radius of the small cylinders. If the two cylinders be of different metals, M will differ in value in the different members of the equation. [To be continued.]

LXV. *On Governor* Ellis's *Discovery of the Action of Cold on Magnetic Needles.*

To the Editors of the Philosophical Magazine and Journal.

Lansdown Crescent, Bath, Nov. 9, 1822.

Gentlemen,—A few days ago, in looking over your publication for last September, my attention was arrested by part of a letter from B. De Sanctis, M.D. "respecting some frigorific experiments made on the magnetic fluid." Their principal result is,—that at certain low temperatures magnetic needles lose their powers, which the writer attributes to the action of cold on the magnetic fluid. Neither the fact, however, nor the explanation is new. Above seventy years ago the fact was made known to the world, and a nearly similar explanation † of it given by Mr. Ellis‡ in the account he published of the voyage undertaken to Hudson's Bay in 1746, in search of a north-west passage. I should not deem this circumstance worth noticing, but for the very extraordinary acknowledgement that it has pleased Dr. De Sanctis to make of the source of his experiments: "I am," says the Doctor, "indebted for the idea of these experiments to *the simple observation* of Captain Ellis, who, meeting ice mountains * in Hudson's Bay, saw his needles sluggish at their approach; and says, he restored them to their former activity by warmth. *Far from reasoning on the circumstances, he has the air of repeating the fact as a kind of mysterious accident.* What a difference between that *rough transient observation* and the results *we* have obtained! Such is the progress of science, aided by time, zeal for experiment, *and skill in observing!*"

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* Essay on Cast-Iron, artt. 81, 90, and 100.

† The chief difference of the explanations is only this: Mr. Ellis conjectured that the action of cold on the needle, as well as on the magnetic particles or fluid, may contribute to produce the effect; while Dr. De Sanctis excludes the action of cold on the needle from any share of the effect. "It is only, or at least principally," he says, "the action of cold on the magnetic fluid itself that produces the paralysis" of the needle; and he intimates that *the magnetic fluid may be frozen into a refined ethereal ice!*

‡ The late Governor Henry Ellis, who died in 1806; and was, for several

After this representation, it will scarcely be credited, without referring to the Voyage †, that so far from *not* having reasoned on the circumstance of the compasses having lost their magnetic properties, and of having the air of repeating the fact *as a kind of mysterious accident,* Mr. Ellis actually occupied above eight pages ‡ with conjectures, reasonings and hypotheses concerning its cause. An attempt was made to restore the powers of the compasses by retouching their needles; but the effectual remedy, *heat,* seems to have been discovered, in consequence of an hypothesis § which has obtained the recent honour of adoption by Dr. De Sanctis, with little variation, except what results from his philosophic idea of *the conversion of the magnetic fluid into refined ethereal ice.*

I am, gentlemen,
Your very obedient servant,
FRANCIS ELLIS.

LXVI. *Description of a new Printing Press.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—THIS press may be used in common printing, lithographic printing, and for various other purposes, and is

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veral years previous to his death, the oldest Member or Father of the Royal Society.

* Neither the approach of ice-mountains, nor ice-mountains, are in any way mentioned. See Ellis's Voyage, page 221.

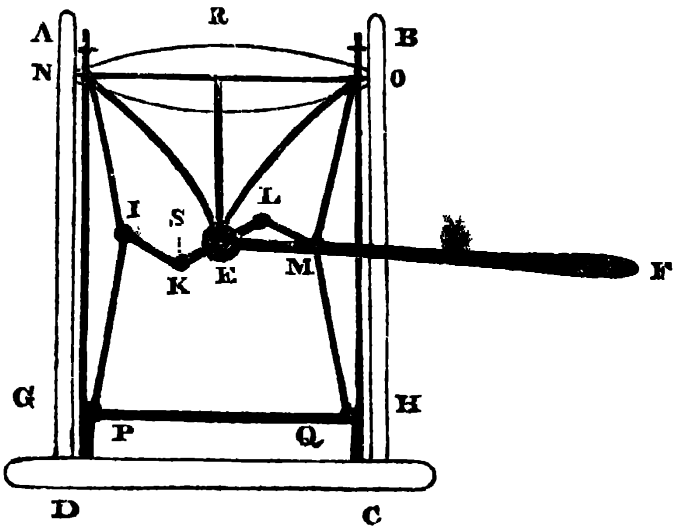
† See Ellis's Voyage from page 220 to page 229.

‡ Namely, from page 220 to page 229 of his Voyage, entitled in the index "Some Thoughts on Magnetism."

§ See Ellis's Voyage, page 226.

equal in effect to any hitherto constructed; such as the Stanhope, Ruthven's, &c. for which patents have been taken out; and much superior in simplicity of construction and cheapness, which will recommend, we hope, its general adoption. It is also easily made by any common blacksmith who is a good workman.

In the adjoining figure, let ABCD be the general frame of the press, connected by the cross pieces NO, DC. E is a centre connected with the frame by the bars EN, ER, EO. To this centre are affixed a bar KL, and a lever EF, to which the hand is applied when the press is used.



There are also several other pieces connected by joints at N, G, I, K, L, M, O, H, which are so adjusted to each other that when the hand is applied to the lever EF at F, by pressing it downwards KL is brought into a horizontal line or parallel to GH or DC, in which situation NIG, OMH, also form each a straight line. It is evident from a long well-known principle in mechanics, that the nearer these different pieces, as above mentioned, are to a straight line, the greater is the lever EF, in proportion to the perpendicular KS at the other end of the lever EK, formed by a perpendicular from K falling on FE produced. (Gregory's *Mechanics*, vol. i. prop. 144. cor. 1.)—Consequently a small force applied at F will be sufficient to produce a very great effect at K, when IK, KE are nearly in a straight line, and so on, for the other pieces above mentioned.

Hence the force applied by hand at F must be very considerable in forcing down GH, which slides on iron cylindrical bars, or in pressing any substance placed in the aperture PQ.

Wherefore the whole theory and principles of the press are apparent. I am, &c.

Edinburgh, Oct. 1822. MECHANICUS.

☞ We think our Correspondent is not aware of the advantages which have already been derived from the above principle; as, with the exception of the original screw press, we do not recollect seeing one to which it has not been applied.

LXVII. *Autumnal Blowing of the Narcissus.*

To the Editors of the Philosophical Magazine and Journal.

Hartwell, Nov. 12, 1822.

GENTLEMEN,—LAST winter I communicated in the Philosophical Magazine*, a catalogue of plants which flowered unseasonably. I have now further to record a very unusual botanical phænomenon. The *Narcissus Tazetta*, or *N. orientalis* of Curtis, being the white Polyanthus Narcissus of the shops, is now in flower in the open ground in my garden at Hartwell in Sussex. This plant was coming into flower at its usual time last April, when the blooms before their expansion were eaten off by the slugs, and the stalk afterwards cut down. If this were the cause of the refloresccence of the plant in November, florists may learn from this accident how to produce an autumnal crop of spring flowers, by cutting them off in spring. Yours,

T. FORSTER.

LXVIII. *On the anomalous magnetic Action of hot Iron between the white and blood-red Heat.* By PETER BARLOW, *Esq. of the Royal Military Academy. Communicated by Major* THOMAS COLBY, *of the Royal Engineers, F.R.S.*†

IN consequence of certain theoretical results relative to the magnetic action of iron, obtained by Mr. Charles Bonnycastle, I was desirous of ascertaining the relative attraction which different species of iron and steel had for the magnet; and with this view I procured two bars of each of the following descriptions of metal, 24 inches in length, and one inch and a quarter square, which being placed successively in the direction of the dip, at a certain distance from the compass, the disturbance occasioned by each was carefully noted; first with one end upwards, and then with the other; and assuming the tangents of the angles as the measure of the disturbing power, I obtained the following specific results, viz.

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* Vol. lix. p. 212.

† From the Transactions of the Philosophical Society, Part I. for 1822.

Malleable

	Mag. Pow.		Mag. Pow.
Malleable iron . . .	100	Shear steel soft . . .	66
Cast iron	48	———— hard . . .	53
Blistered steel soft .	67	Cast steel soft	74
————hard	53	———— hard	49

As it was obvious from these experiments, that the softer the iron the greater was its power, and the contrary, I was desirous of determining how nearly these different kinds of metal would approximate towards each other in their magnetic action, when rendered perfectly soft by being heated in a furnace. With this view, bars of equal size of cast-iron, malleable iron, shear steel, &c. were rendered white hot, and being placed in the direction of the dip, as before, their powers, as was anticipated, agreed nearly with each other; but still the cast iron, which was weakest while the metal was cold, exceeded a little in power all the others when hot, and the malleable iron which had the greatest power cold, had the least when hot; but the difference was not very great, and might probably arise from some accidental circumstance. While carrying on these experiments, it had been observed, both by Mr. Bonnycastle and myself, that between the white heat of the metal, when all magnetic action was lost, and the blood-red heat, at which it was the strongest, there was an intermediate state in which the iron attracted the needle the contrary way to what it did when it was cold, viz. if the bar and compass were so situated that the *north* end of the needle was drawn towards it when cold, the *south* end was attracted during the interval above alluded to, or while the iron was passing through the shades of colour denoted by the workman the bright red and red heat.

As this anomalous action had never before been noticed, I was desirous of examining it a little more particularly; and with the assistance of Mr. Bonnycastle, the following series of experiments were performed, wholly directed to this inquiry. Before entering upon the detail, however, it may not be amiss to notice those results which have hitherto been obtained relative to the magnetic action of heated iron; and to show how the contradictory statements, that we find on the subject, may be reconciled with each other. For example: we find it stated in Newton's Optics, that red hot iron has no magnetic property; while Father Kircher asserts that the magnet will attract red hot iron as well as cold, ("de Magnete," lib. 1). Again, in Vol. xviii. No. 214. Phil. Trans. it is stated, that hot iron not only has an attraction for the magnet, but that its power is increased by the heat; and these assertions have been repeated by many other authors, each supposing that his results were at variance with the other.

M. Cavallo seems to have been the first writer, who was fully aware that these contradictory statements arose from the observations being made with the iron at different degrees of heat. He found, that although iron at the red heat had a greater power over the magnet than when cold, yet, at the white heat, it had a less; but he says he is still unable to decide, whether all the magnetic power is intirely lost at the white heat. (Cavallo on Magnetism, p. 312). More recent experiments on this subject are also recorded in vol. ix. Part I. of the Transactions of the Royal Society of Edinburgh, by William Scoresby, Esq. But even here it does not appear that this gentleman was aware of the total loss of power at a certain temperature; for he observes (after showing that iron red hot has a greater power than when cold).; "The contrary to this has, I think, been generally asserted;" from which it would seem, that he had not heated his iron to a sufficient degree to detect the non-action at the white heat.

Notwithstanding, therefore, all the experiments that have been made, it is pretty evident from the above remarks, that considerable uncertainty still hangs over the results; arising, without doubt, from the want of proper conveniences for heating bars of sufficient size, and to a proper degree of intensity; whereby one author has noticed one fact, and another a different one, without being aware how much depended upon a very slight change in the temperature of the iron.

On these grounds therefore it is presumed, that the following experiments would be entitled to some notice, as they serve to reconcile all these apparently contradictory statements; but the principal reason which has induced me to lay them before the Royal Society is, the anomalous action which they have been the means of discovering, while the iron passes through the shades of bright red and red, already alluded to in the preceding part of this paper, and which, to the best of my knowledge, has never been noticed by any author.

Experiments on the anomalous Attraction of heated Iron which takes place while the Metal retains the bright red and red heat.

I have already observed, that this anomalous action was noticed while we were pursuing other experiments, and that those which follow, were wholly directed to an examination of these irregularities.

In our first experiment, the compass was placed nearly west of the bar, rather below its upper extremity, and distant from it about 6½ inches. At the white heat the attraction of the iron was lost; and at the blood-red heat we had 70° of deviation in the needle; but that intermediate action we were searching after did not appear; at least it was by no means so obvious as we had noticed it in our preceding experiments.

The position of the bar and compass was not however quite the same as before; we therefore raised the support of the bar about four inches, by which means its upper extremity was the same height above the compass: and on repeating the experiment with the bar thus placed, we obtained an obvious deviation of the south end of the needle towards the iron of 4½°, which remained fixed about two minutes.

Having gained this by raising the bar four inches, we now raised it six inches; and on applying it in this place, we obtained a deviation of 10½°, which remained fixed about the same time as before; when the needle suddenly yielded to the natural magnetic power of the iron, and obtained almost instantaneously a deviation of 81° the opposite way.

It was thus rendered obvious, that the quantity of negative attraction at the red heat, depended upon the height or depth of the centre of the bar from the compass; and as the natural effect of the cold iron was changed by placing the compass below the centre of the bar, the question which naturally suggested itself was, Will the negative attraction also change? To decide this point, we lowered our compass to within six inches of the bottom of the bar; in which position the cold iron necessarily attracted the south end of the needle, and produced a deviation of 21°. Upon heating the bar, we found, as usual, all its power upon the needle cease at the white heat; but as this subsided into the bright red, the negative attraction began to manifest itself, and it soon amounted to 10½°; the north end of the needle being attracted towards the iron. Here it remained stationary a short time, and then gradually returned, first due north, and ultimately to 70° 30′ on the opposite side.

Having made these preliminary experiments, I was anxious to undertake a regular series, hoping by this means to be able to reduce this species of action to some fixed principle; for it will have been observed, from what is stated above, that the negative attraction appeared to increase from each extremity of the bar towards its middle; whereas the positive or natural action of the iron decreases in the like cases, and (passing through zero in the plane of no attraction) has its quantity of attraction different when placed towards the upper or lower extremity of the bar.

The negative attraction has also the same change of character in the upper and lower extremity of the bar; but as it increases towards the middle, it appeared to pass through a maximum to arrive at that change, which seemed wholly inexplicable; and I must acknowledge that, after all the experiments I have made, it still remains so. It is at all events certain, that the least change of position of the compass when near the centre of the bar, changes altogether the quantity and quality of this negative action.

In the experiments detailed in the following table, I used four different bars, each 25 inches long, and 1¼ inch square; two of them of cast iron, denoted in the first column by C. B, No. 1; C. B, No. 2; and two of malleable iron, denoted by M. B, No. 1, and M. B, No. 2.

I had also two other bars, one of cast and one of malleable iron, of the same dimensions, which were not heated, but kept as standards for determining the quantity of cold attraction, as this could not safely be done by the bars used in the experiments after being so repeatedly heated.

The time employed in each experiment was about a quarter of an hour: the white heat commonly remained about three minutes, when the negative attraction commenced; this lasted about two minutes more, when the usual attraction took place: this sometimes arrived at its maximum very rapidly, but at others it proceeded increasing very gradually; and commonly within the time stated above, the needle had been found perfectly stationary.

In the table, to avoid confusion, that attraction which took place according to the known laws of cold iron is marked *plus*, whichever end of the needle approached the iron, and the opposite attraction is marked *minus*. For example: when the compass is above the centre of the bar, the north end of the needle should be drawn towards the iron; but when the compass is below the centre, the south end should approach the iron; these therefore are both marked *plus*, and the contrary attraction at the red heat is marked *minus*.

TABLE

Showing the Effect of Iron on the Compass Needle at different Degrees of Heat.

No.	Description of bar.	Height or depth of centre of bar from compass.	Distance of bar from compass.	Position of compass.	Effect cold.	Effect white heat.	Effect red heat.	Effect blood red heat.	Remarks.
		Inch.	Inch.	°	° '	° '	° '	° '	
1	C.B. No. 1	0·0	6·0	S. 80 W.	+ 0 0	0 0	−17 0	0 0	South end drawn to the bar at red heat.
2	M.B. No. 2	4·5 below	6·0	ditto	+30 0	ditto	0 0	+15 0	
3	C.B. No. 2	4·5 below	6·0	ditto	+18 0	ditto	0 0	+49 0	
4	M.B. No. 1	ditto	6·0	ditto	+29 30	ditto	−12 0	+44 0	
5	ditto	13 below	6·0	ditto	Not obs.	ditto	0 0	+52 0	
6	ditto	4·5 below	6·0	N. 80 W.	ditto	ditto	−12 30	+70 0	This bar being left standing, it attracted the same three days after.
7	ditto	1·5 above	6·0	S. 80 W.	ditto	ditto	−12 30	+30 0	
8	ditto	ditto	6·0	ditto	ditto	ditto	0 0	+25 0	The needle suspected to touch the box.
9	ditto	ditto	6·0	ditto	ditto	ditto	−19 0	+30 0	
10	ditto	1·0 above	6·0	ditto	ditto	ditto	−15 0	+ 4 0	
11	M.B. No. 2	12·5 below	8·5	N. 80 W.	+29 30	ditto	0 0	+37 30	Observed at the same time with two compasses.
12	ditto	ditto	8·5	N. 80 E.	+30 0	ditto	0 0	+41 0	
13	C.B. No. 1	12·5 below	8·5	N. 80 W.	+16 0	ditto	0 0	+42 30	Ditto.
14	ditto	ditto	8·5	N. 80 E.	+15 30	ditto	0 0	+47 30	

15	M.B. No. 2	9·0 below	8·5	N. 80 W.	+28 30	0° 0′	− 1 0	+39 30	Observed at the same time with two compasses.
16	ditto	ditto	8·5	N. 80 E.	+29 30	ditto	− 1 30	+42 0	
17	C.B. No. 1	9·0 below	8·5	N. 80 W.	+15 45	ditto	− 1 30	+45 0	Ditto.
18	ditto	ditto	8·5	N. 80 E.	+16 0	ditto	− 1 30	49 0	
19	M.B. No. 2	6·0 below	8·5	N. 80 W.	+25 0	ditto	− 3 0	+32 30	Ditto.
20	ditto	ditto	8·5	N. 80 E.	+26 0	ditto	− 3 30	+33 0	
21	C.B. No. 1	6·0 below	8·5	N. 80 W.	+11 30	ditto	− 3 30	+36 30	Ditto.
22	ditto	ditto	8·5	N. 80 E.	+13 0	ditto	Not obs.	+36 30	
23	M.B. No. 2	3·0 below	6·0	S. 80 E.	+ 8 0	ditto	− 21 30	Not obs.	Ditto.
24	ditto	ditto	6·0	N. 45 W.	Not obs.	ditto	− 25 30	+25 30	
25	M.B. No. 1	0·0	6·0	ditto	0 0	ditto	− 40 0	0 0	North end drawn to the bar at red heat.
26	M.B. No. 2	1·0 above	5·3	N. 60 W.	+ 2 0	ditto	− 4 30	+ 5 30	Both attractions very gradual.
27	M.B. No. 1	ditto	5·3	ditto	Not obs.	ditto	− 12 30	+ 5 30	Passed suddenly to 12½°, but returned immediately.
28	M.B. No. 2	9·0 above	6·0	N. 85 E.	+47 30	ditto	− 2 30	+60 0	Attractions gradual.
29	M.B. No. 1	ditto	6·0	ditto	+47 30	ditto	− 2 30	+60 0	Ditto.
30	M.B. No. 2	1·0 below	5·5	N. 45 W.	Not obs.	ditto	− 55 0	+ 5 45	Negative attraction rather sudden.
31	M.B. No. 1	4·5 above	7·0	N. 75 E.	ditto	ditto	− 2 30	+33 30	Motion of needle very slow.
32	M.B. No. 2	1·7 below	5·5	N. 45 W.	ditto	ditto	+100 0	+13 30	100° very sudden, returned immediately.
33	M.B. No. 1	1·7 above	5·5	ditto	ditto	ditto	− 26 0	+13 30	Both attractions gradual.
34	M.B. No. 2	1·7 above	5·5	ditto	ditto	ditto	+ 30 0	+13 30	The same as No. 32; both anomalous.
35	M.B. No. 1	4·5 above	6·0	N. 55 E.	ditto	ditto	− 5 30	+35 30	Attractions very gradual.
36	M.B. No. 2	ditto	6·0	ditto	ditto	ditto	− 0 0	+35 30	
37	M.B. No. 1	0·0	4·7	West	+ 3 30	ditto	− 50 0	+ 8 0	Motion regular, but quick.
38	M.B. No. 2	0·0	4·7	North.	0 0	ditto	0 0	0 0	No motion in the needle.

It should be observed, that all the above experiments were made with the bars inclined in the direction of the dipping needle, or nearly in that direction, and it will be seen that the negative attraction was the greatest where the natural attraction was the least; that is, opposite the middle of the bar, or in the plane of no attraction. I was led, therefore, to make a few experiments with the bar inclined at right angles to its former position, but the results were by no means so strongly marked as in the preceding experiments: we always found a certain quantity of negative attraction, but it was very inconsiderable, never amounting to more than $2\frac{1}{2}°$.

I also made one experiment with an iron 24lb. ball, but the heat was too intense to make any very accurate observation. The numbers obtained were

Cold attraction $+13° 30'$	White heat $0° 0'$.
Red heat $-3° 30'$.	Blood-red heat $+19° 30'$.

It may be proper also to state, that, being doubtful how far the heat itself, independent of the iron, might be the cause of the anomalous action above described, I procured two copper bolts of rather larger dimensions than my iron bars, and had them heated to the greatest degree that metal would bear; but on applying them to the compass, no motion whatever could be discovered in the needle.

The only probable explanation which I can offer by way of accounting for these anomalies, is, that the iron cooling faster towards its extremities than towards its centre, a part of the bar will become magnetic before the other part, and thereby cause a different species of attraction; but I must acknowledge, that this will not satisfactorily explain all the observed phænomena. The results, however, are stated precisely as they were noted during the experiments, and others more competent than myself will probably be able to deduce the theory of them.

LXIX. *Experiments on the Smut in Wheat.* *By Mr.* SAMUEL TAYLOR, *of Bungay.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—OF the infectious nature of the disease called smut in wheat, I have long been convinced from a series of experiments and observations which cannot have deceived me. By smut I wish to be clearly understood to mean that kind which shows itself in small roundish balls or bladders, nearly similar in appearance, when growing, to ears of wheat: but which, on being broken, are found to contain a black subtile powder, extremely fetid and disagreeable to the smell. I am thus particular in describing the disease, because most writers on the diseases of wheat have made strange confusion on the subject, having jumbled together smut, mildew, pepperbrand; in short, all the diseases to which this grain is liable, under one denomination; whereas nothing can be more distinct. The smut, brand, or collibrand, is perhaps the most generally known: and is produced, I believe, entirely by infection,—I mean by the breaking of the balls containing the powder, and by the rubbing of such powder against sound grains. It is in the power of any man to produce a smutty crop; but, what is of vast importance to know, it is equally in his power to produce a clean crop, even from seed which has been previously infected. It is not my intention at present to go into the merits of particular preparations for this purpose; but the fact is well known to every practical farmer, that proper care in washing, brining and liming seed wheat will counteract the infectious qualities of the smut powder, and will generally ensure a clean crop.

The pepperbrand is a disorder perfectly distinct from the foregoing. When growing, it is distinguishable from healthy ears by the chaff (instead of adhering to the grain closely and regularly) starting nearly at right angles with the stem and presenting an uneven spiky appearance; the intervals between the *koshes* or outside coating of the grain being open so as to admit a sight of the diseased grain within. This latter is a hard round substance, not unlike either in shape or colour to a peppercorn, and having no apparent affinity to a grain of wheat. From experiments which I have made, I have no hesitation in saying that this disease is also propagable by infection, or by the contact of the pepper balls with sound wheat. I am also equally certain that careful washing, steeping, and liming will restore the infected grain to health: this will appear the more extraordinary when the following circumstances are taken into consideration. A parcel of fine clean wheat (consisting of about three pints) was well rubbed by me with the balls of this same pepperbrand; one half of which was immediately sown in rows in a plot of ground prepared for the purpose, No. 2; the other half, No. 1, was then washed, steeped about half an hour in a solution of green vitriol and water, limed, and sown in rows by the side of the former half. To my surprise, there was a striking difference in the *appearance* of the plants of each parcel throughout the winter, which difference continued to increase till harvest. The infected, *unwashed* parcel was diseased not only in the ears, but the whole plant was distorted, and at the time of cutting was not much more than one-half the height of the other. The subjoined sketch will better explain the state of each than mere words can do.

Nearly the whole of No. 2 was pepperbrand: No. 1 was entirely free from it. Each had been rubbed in a similar manner, only that in the one case infection had been stopped; in the other it had been suffered to take its course. The extraordinary part of the business is, first, that a hard substance like pepperbrand should possess infectious qualities at all, and next, that that infection should extend not to the ear merely, but to the whole plant.

No. 1.

No. 2.

I wish some of your correspondents would but try the experiment and communicate the result; but let it be done with the strictest attention and care, or it will be worse than useless. Of the necessity of regard to minutiæ in such matters, surely there can be no doubt. Every thing hinges on it.

Mildew in wheat has been so ably and so fully described in Sir Joseph Banks's very elaborate and scientific work* on that subject, that any thing but a reference to the book itself must be superfluous on the present occasion. Suffice it to say, it has nothing to do with smut of any kind, nor, I believe, are any kind of steeps available to prevent it. I assert this with confidence; although in opposition to the recorded opinions both of Sir John Sinclair, and Dr. Cartwright; the former of whom some few years since strongly recommended steeping the seed in a solution of sulphate of copper, but latterly seems to have changed his mode of attack, by coalescing with the worthy Doctor, and advancing to lay salt on the tail of the enemy. It ought to be observed, however, that Sir John applies himself *à priori* to the soil, and recommends a dressing of from 30 to 40 bushels of salt per acre: whilst the Doctor sallies forth, pot in hand, to besprinkle the plants with salt and water as they grow. I believe pressure of the soil to be the best remedy; for which reason I have always thought highly of the dibble husbandry. To prove the utility of consolidating the soil by well treading it, I once took the pains to tread very carefully one half of an experimental patch of ground which had been sown with wheat in rows, and the soil of which was rather loose: the treading was done not lengthwise but *across*

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* See Phil. Mag. vol. xxi. p. 325.

LXXV. *Experiments on the Alloys of Steel, made with a view to its Improvement.* By J. STODART, *Esq. F.R.S.* and M. FARADAY, *Chemical Assistant at the Royal Institution*[*].

IN proposing a series of experiments on the alloys of iron and steel, with various other metals, the object in view was two-fold: first, to ascertain whether any alloy could be artificially formed, better for the purpose of making cutting-instruments than steel in its purest state; and, secondly, whether any such alloys would, under similar circumstances, prove less susceptible of oxidation; new metallic combinations for reflecting mirrors was also a collateral object of research.

Such a series of experiments were not commenced without anticipating considerable difficulties, but the facilities afforded us in the laboratory of the Royal Institution, where they were made, have obviated many of them. The subject was new, and opened into a large and interesting field. Almost an infinity of different metallic combinations may be made, according to the nature and relative proportions of the metals capable of being alloyed. It never has been shown, by experiment, whether pure iron, when combined with a minute portion of carbon, constitutes the very best material for making edge-tools; or whether any additional ingredient, such as the earths or their bases, or any other metallic matter, may not be advantageously combined with the steel; and, if so, what the materials are, and what the proportion required to form the best alloy for this much-desired and most important purpose. This is confessedly a subject of difficulty, requiring both time and patient investigation, and it will perhaps be admitted as some apology for the very limited progress as yet made.

By referring to the analysis of wootz, or Indian steel[†], it will be observed, that only a minute portion of the earths alumine and silex, could be detected, these earths (or their bases) giving to the wootz its peculiar character. Being satisfied as to the constituent parts of this excellent steel, it was proposed to attempt making such a combination, and, with this view, various experiments were made. Many of them were fruitless: the successful method was the following. Pure steel in small pieces, and in some instances good iron, being mixed with charcoal powder, were heated intensely for a long time; in this way they formed carburets, which possessed a very dark metallic grey colour, something in appearance like the black ore of tellurium, and highly crystalline. When broken, the facets of small buttons, not weighing more than 500 grains, were frequently above the eighth of an inch in

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[*] From the Journal of Sciences and the Arts.

[†] *Ibid.* vol. vii. p. 288.

which

width. The results of several experiments on its composition, which appeared very uniform, gave 94·36 iron, + 5·64 carbon. This being broken and rubbed to powder in a mortar, was mixed with pure alumine, and the whole intensely heated in a close crucible for a considerable time. On being removed from the furnace, and opened, an alloy was obtained of a white colour, a close granular texture, and very brittle: this, when analysed, gave 6·4 *per cent.* alumine, and a portion of carbon not accurately estimated. 700 of good steel, with 40 of the alumine alloy, were fused together, and formed a very good button, perfectly malleable; this, on being forged into a little bar, and the surface polished, gave, on the application of dilute sulphuric acid, the beautiful damask which will presently be noticed as belonging peculiarly to wootz. A second experiment was made with 500 grains of the same steel, and 67 of the alumine alloy, and this also proved good; it forged well, and gave the damask. This specimen has all the appreciable characters of the best Bombay wootz.

We have ascertained, by direct experiment, that the wootz, although repeatedly fused, retains the peculiar property of presenting a damasked surface, when forged, polished, and acted upon, by dilute acid. This appearance is apparently produced by a dissection of the crystals by the acid; for though by the hammering the crystals have been bent about, yet their forms may be readily traced through the curves, which the twisting and hammering have produced. From this uniform appearance on the surface of wootz, it is highly probable, that the much-admired sabres of Damascus are made from this steel; and, if this be admitted, there can be little reason to doubt, that the damask itself is merely an exhibition of crystallization. That on wootz it cannot be the effect of the mechanical mixture of two substances, as iron and steel, unequally acted upon by acid, is shown by the circumstance of its admitting re-fusion, without losing this property. It is certainly true, that a damasked surface may be produced by welding together wires of iron and steel; but if these welded specimens are fused, the damask does not again appear. Supposing that the damasked surface is dependent on the developement of a crystalline structure, then the superiority of wootz in showing the effect, may fairly be considered as dependent on its power of crystallizing, when solidifying, in a more marked manner, and in more decided forms, than the common steel. This can only be accounted for by some difference in the composition of the two bodies; and as it has been stated that only the earths in small quantities can be detected, it is reasonable to infer, that the bases of these earths being combined with the iron and carbon render the mass more crystallizable, and that the structure drawn out by the hammer, and confused (though not destroyed), does actually occasion the damask. It is highly probable, that the wootz is steel, accidentally combined with the metal of the earths, and the irregularity observed in different cakes, and even in the same cake, is in accordance with this opinion. The earths may be in the ore, or they may be derived from the crucible in which the fusion is made.*

It will appear by the following experiment, that we had formed artificial wootz, at a time when this certainly was not the object of research. In an attempt to reduce titanium, and combine it with steel, a portion of menachanite was heated with charcoal, and a fused button obtained. A part of this button was next fused with some good steel; the proportions were 96 steel, 4 menachanite button. An alloy was formed, which worked well under the hammer; and the little bar obtained was evidently different from, and certainly superior to, steel. This was attributed to the presence of titanium, but none could be found in it; nor indeed was any found even in the menachanite button itself. The product was iron and carbon, combined with the earths or their bases, and was in fact excellent wootz. A beautiful damask was produced on this specimen by the action of dilute acid. Since this, many attempts have been made to reduce the oxide of titanium; it has been heated intensely with charcoal, oil, &c., but hitherto all have failed, the oxide has been changed into a black powder, but not fused. When some of the oxide was mixed with steel filings, and a little charcoal added, on being intensely heated the steel fused, and ran into a fine globule which was covered by a dark coloured transparent glass, adhering to the sides of the crucible. The steel contained no titanium, the glass proved to be oxide of titanium, with a little oxide of iron. These experiments have led us to doubt whether titanium has ever been reduced to the metallic state. From the effects of the heat upon the crucibles, which became soft, and almost fluid, sometimes in fifteen minutes, we had in fact no reason to suppose the degree of heat inferior to any before obtained by a furnace:

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* In making the alumine alloy for the imitation of wootz, we had occasion to observe the artificial formation of plumbago. Some of the carburet of iron before-mentioned having been pounded and mixed with fresh charcoal, and then fused, was found to have been converted into perfect plumbago. This had not taken place throughout the whole mass; the metal had soon melted, and run to the bottom; but having been continued in the furnace for a considerable time, the surface of the button had received an additional portion of charcoal, and had become plumbago It was soft, sectile, bright, stained paper, and had every other character of that body; it was indeed in no way distinguishable from it. The internal part of these plumbago buttons was a crystalline carburet: a portion of it having been powdered, and fused several times with charcoal, at last refused to melt, and on the uncombined charcoal being burnt away by a low heat, it was found that the whole of the steel had been converted into plumbago; this powder we attempted to fuse, but were not successful.

—that used in these last experiments was a blast furnace, supplied by a constant and powerful stream of air; the fuel good Staffordshire coke, with a little charcoal; both Hessian and Cornish crucibles were used, one being carefully luted into another,—and even three have been united, but they could not be made to stand the intense heat.

Meteoric iron is, by analysis, always found to contain nickel. The proportions are various, in the specimens that have been chemically examined. The iron from the Arctic regions was found to contain three *per cent.* only of nickel, while that from Siberia gave nearly 10 *per cent.* With the following analysis of this last we are favoured by J. G. Children, Esq.

37 grains of Siberian meteoric iron gave 48·27 grains of peroxide of iron, and 4·52 grains of oxide of nickel. Supposing the equivalent number for nickel to be 28, these quantities are equal to

Iron 33·69, Nickel 3·56, = 37·25

Supposing the quantities to be correctly, Iron 33·5, Nickel 3·5 = 37·0, the proportions *per cent.* are, Iron 90·54, Nickel 9·46, = 100·00.

A second experiment on 47 grains, gave 61 grains of peroxide of iron =42·57 iron. The ammoniacal solution of nickel was lost by an accident: reckoning from the iron, the quantities *per cent.* are,

Iron 90·57, Nickel 9·42, = 99·99

A third experiment on 56 grains, gave 73·06 grains peroxide of iron = 50·99 iron, and 5·4 of oxide of nickel, = 4·51 nickel, or *per cent.*

Iron 91.00. Nickel 8·01. Loss 0·99 = 100·00

The mean of the three gives 8·96 *per cent.* of *nickel.*

The meteoric iron was dissolved in aqua regia, and the iron thrown down by pure ammonia, well washed, and heated red.

In the first experiment the ammoniacal solution was evaporated to dryness, the ammonia driven off by heat, and the oxide of nickel re-dissolved in nitric acid, and precipitated by pure potassa, the mixture being boiled a few seconds. In the third experiment the nickel was thrown down from the ammoniacal solution at once by pure potassa. The first method is best, for a minute portion of oxide of nickel escaped precipitation in the last experiment, to which the loss is probably to be attributed. All the precipitates were heated to redness. J. G. C.

We attempted to make imitations of the meteoric irons, with perfect success. To some good iron (horse-shoe nails), were added three *per cent.* of pure nickel; these were inclosed in a crucible, and exposed to a high temperature in the air-furnace for some hours. The metals were fused, and on examining the button, the nickel was found in combination with the iron. The alloy was taken to the forge, and proved under the hammer to be quite as malleable and pleasant to work as pure iron; the colour when polished rather whiter. This specimen, together with a small bar of meteoric iron, have been exposed to a moist atmosphere; they are both a little rusted. In this case it was omitted to expose a piece of pure iron with them; it is probable that, under these circumstances, the pure iron would have been more acted upon.

The same success attended in making the alloy to imitate the Siberian meteoric iron agreeably to Mr. Children's analysis. We fused some of the same good iron, with 10 *per cent.* nickel; the metals were found perfectly combined, but less malleable, being disposed to crack under the hammer. The colour when polished had a yellow tinge. A piece of this alloy has been exposed to moist air for a considerable time, together with a piece of pure iron; they are both a little rusted, not, however, to the same extent; that with the nickel being but slightly acted upon, comparatively to the action on the pure iron. It thus appears that nickel, when combined with iron, has some effect in preventing oxidation, though certainly not to the extent that has at times been given to it. It is a curious fact, that the same quantity of the nickel alloyed with steel, instead of preventing its rusting, appeared to accelerate it very rapidly.

Platinum and rhodium have, in the course of these experiments, been alloyed with iron, but these compounds do not appear to possess any very interesting properties. With gold we have not made the experiment. The alloys of other metals with iron, as far as our experience goes, do not promise much usefulness. The results are very different when steel is used; it is only, however, of a few of its compounds that we are prepared to give any account.

Together with some others of the metals, the following have been alloyed with both English and Indian steel, and in various proportions: platinum, rhodium, gold, silver, nickel, copper, and tin.

All the above-named metals appear to have an affinity for steel sufficiently strong to make them combine; alloys of platinum, rhodium, gold and nickel, may be obtained when the heat is sufficiently high. This is so remarkable with platinum, that it will fuse when in contact with steel, at a heat at which the steel itself is not affected.

With respect to the alloy of silver, there are some very curious circumstances attending it. If steel and silver be kept in fusion together for a length of time, an alloy is obtained, which appears to be very perfect while the metals are in the fluid state, but on solidifying and cooling, globules of pure silver are expressed from the mass, and appear on the surface of the button. If an alloy of this kind be forged into a bar, and then dissected by the action of dilute sulphuric acid, the silver appears, not in combination with the steel, but in threads throughout the mass; so that the whole has the appearance of a bundle of fibres of silver and steel, as if they had been united by welding. The appearance of these silver fibres is very beautiful; they are sometimes one-eighth of an inch in length, and suggest the idea of giving mechanical toughness to steel, where a very perfect edge may not be required.

“At other times, when silver and steel have been very long in a state of perfect fusion, the sides of the crucible, and frequently the top also, are covered with a fine and beautiful dew of minute globules of silver:—this effect can be produced at pleasure. At first we were not successful in detecting silver by chemical tests in these buttons; and finding the steel uniformly improved, were disposed to attribute its excellence to an effect of the silver, or to a quantity too small to be tested. By subsequent experiments we were, however, able to detect the silver, even to less than 1 in 500.

In making the silver alloys, the proportion first tried was 1 silver to 160 steel; the resulting buttons were uniformly steel and silver in fibres, the silver being likewise given out in globules during solidifying, and adhering to the surface of the fused button; some of these when forged gave out more globules of silver. In this state of mechanical mixture the little bars, when exposed to a moist atmosphere, evidently produced voltaic action; and to this we are disposed to attribute the rapid destruction of the metal by oxidation, no such destructive action taking place when the two metals are chemically combined. These results indicated the necessity of diminishing the quantity of silver, and 1 silver to 200 steel was tried. Here, again, were fibres and globules in abundance; with 1 to 300 the fibres diminished, but still were present; they were detected even when the proportion of 1 to 400 was used. The successful experiment remains to be named. When 1 of silver to 500 steel were properly fused, a very perfect button was produced; no silver appeared on its surface; when forged and dissected by an acid, no fibres were seen, although examined by a high magnifying power. The specimen forged remarkably well, although very hard; it had in every respect the most favourable appearance. By a delicate test every part of the bar gave silver. This alloy is decidedly superior to the very best steel, and this excellence is unquestionably owing to combination with a minute portion of silver. It has been repeatedly made, and always with equal success. Various cutting-tools have been made from it of the best quality. This alloy is perhaps only inferior to that of steel with rhodium, and it may be procured at a small expense; the value of silver, where the proportion is so small, is not worth naming; it will probably be applied to many important purposes in the arts. An attempt was made to procure the alloy of steel with silver by cementation; a small piece of steel wrapped in silver leaf, being 1 to 100, was put into a crucible, which being filled up with pounded green glass, was submitted to a heat sufficient to fuse the silver; it was kept at a white heat for three hours. On examining it, the silver was found fused, and adhering to the steel; no part had combined. The steel had suffered by being so long kept at a high temperature. Although this experiment failed in effecting the alloy of steel with silver, there is reason to believe that with some other metals, alloys may be obtained by this process;—the following circumstance favours this suggestion. Wires of platinum and steel, of about equal diameter, were packed together, and, by an expert workman, were perfectly united by welding. This was effected with the same facility as could have been done with steel and iron. On being forged, the surface polished, and the steel slightly acted on by an acid, a very novel and beautiful surface appeared, the steel and platinum forming dark and white clouds; if this can be effected with very fine wires, a damasked surface will be obtained, of exquisite beauty. This experiment, made to ascertain the welding property of platinum, is only named here in consequence of observing that some of the largest of the steel clouds had much the appearance of being alloyed with a portion of the platinum. A more correct survey of the surface, by a high magnifying power, went far to confirm this curious fact: some more direct experiments are proposed to be made on this apparent alloy by cementation.

The alloys of steel with platinum, when both are in a state of fusion, are very perfect, in every proportion that has been tried. Equal parts by weight form a beautiful alloy, which takes a fine polish, and does not tarnish; the colour is the finest imaginable for a mirror. The specific gravity of this beautiful compound is 9·862.

90 of platinum with 20 of steel, gave also a perfect alloy, which has no disposition to tarnish, the specific gravity 15·88; both these buttons are malleable, but have not yet been applied to any specific purpose.

10 of platinum to 80 of steel, formed an excellent alloy. This was ground, and very highly polished, to be tried as a mirror; a fine damask, however, renders it quite unfit for that purpose.

The proportions of platinum that appear to improve steel for edge-instruments, are from 1 to 3 *per cent.* Experience does not yet enable us to state the exact proportion that forms the best possible alloy of these metals; 1·5 *per cent.* will probably be very nearly right. At the time of combining 10 of platinum with 80 steel, with a view to a mirror, the same proportions were tried with nickel and steel; this too had the damask, and consequently was unfit for its intention. It is curious to observe the difference between these two alloys, as to susceptibility for oxygen. The platinum and steel, after laying many months, had not a spot on its surface, while that with nickel was covered with rust; they were in every respect left under similar circumstances. This is given as an instance, showing that nickel with steel is much more subject to oxidation than when combined with iron.

The alloys of steel with rhodium are likely to prove highly valuable. The scarcity of that metal must, however, operate against its introduction to any great extent.

It is to Dr. Wollaston we are indebted, not only for suggesting the trial of rhodium, but also for a liberal supply of the metal, as well as much valuable information relative to fuel, crucibles, &c.; this liberality enables us to continue our experiments on this alloy. The proportions we have used are from 1 to 2 *per cent.* The valuable properties of the rhodium alloys are hardness, with sufficient tenacity to prevent cracking either in forging or in hardening. This superior hardness is so remarkable, that in tempering a few cutting-articles made from the alloy, they required to be heated full 30° Fahr. higher than the best wootz, wootz itself requiring to be heated full 40° above the best English cast steel. Thermometrical degrees are named, that being the only accurate method of tempering steel.

Gold forms a good alloy with steel. We are yet enabled to speak of its properties. It does not promise to be of the same value as the alloys of silver, platinum, and rhodium.

Steel with two *per cent.* of copper, forms an alloy. Steel also alloys with tin. Of the value of these we have doubts. If, on further trial, they, together with other combinations requiring more time than we have been able to bestow on them, should prove at all likely to be interesting and useful, the results will be frankly communicated.

Our experiments have hitherto been confined to small quantities of the metals, seldom exceeding 2,000 grains in weight; and we are aware that the operations of the laboratory are not always successful when practised on a large scale. There does not, however, appear to be any good reason why equal success may not attend the working on larger masses of the metals, provided the same diligence and means are employed.

From the facility of obtaining silver, it is probable that its alloy with steel is the most valuable of those we have made. To enumerate its applications, would be to name almost every edge-tool. It is also probable that it will prove valuable for making dies, especially when combined with the best Indian steel.

Specific Gravities of Alloys, &c. mentioned in the preceding Paper.

Iron, unhammered	7·847	Steel, & 10 *per cent* nickel (mirror)	7·384
Wootz, unhammered (Bombay)	7·665	Steel, & 1 *per ct.* gold, hammered	7·870
Wootz, tilted (Bombay)	7·670	Steel, & 2 *per ct.* silver, ditto ...	7·808
Wootz, in cake (Bengal)	7·730	Steel, & 1·5 *per ct.* platinum, ditto	7·732
Wootz, fused & ham^d. (Bengal)	7·787	Steel, & 1·5 *per ct.* rhodium, ditto	7·795
Meteoric iron, hammered	7·965	Steel, & 3 *per ct.* nickel, ditto ...	7·750
Iron, and 3 *per cent* nickel ...	7·804	Platinum 50, & steel 50, unha^d.*	9·862
Iron, and 10 *per cent* nickel ...	7·849	Platinum 90, & steel 20, ditto†	15·88
Steel, & 10 *do.* platinum (mirror)	8·100	Platinum, hammered and rolled	21·25

* The calculated mean specific gravity of this alloy is 11·2723, assuming the specific gravity of platinum and steel, as expressed in this table.

† The calculated mean specific gravity of this alloy is 16·0766.

LXXVI. *On*

the rows; the result proved highly in favour of the consolidating system; that part which had been trodden was not in the least mildewed; the untrodden was very much so.

The species of smut called in this county powderbrand or blind ears, though certainly unsightly, is not so injurious as it appears: I do not think it infectious; though I have taken some pains to inoculate good seed with the powder. The first heavy rain generally washes it off the stem on which it was produced. It is more prevalent in white than in red wheat; but it is likewise frequently found both in barley and oats: careful washing in two or three waters will prevent the spread of the disease.

I have been induced to send you the above, in the hope, as before observed, that it may lead to discussion and practical observation amongst such of your readers as may have a taste for agricultural pursuits; and believe me to remain,

Gentlemen,
Your most obedient humble servant,

Bungay, Oct. 22, 1822. S. TAYLOR.

LXX. *Description of* Hemipodius nivosus; *a new Bird from Africa. By* WILLIAM SWAINSON, *Esq. F.R.S. and L.S. M.W.S. &c.**

THE singular birds forming the genus *Hemipodius*, are inhabitants of the African and Asiatic continent. Their usual size is so remarkably small, that they may be considered the pygmies of the gallinaceous tribe; between which, and the *Grallæ* they appear to hold an intermediate situation. Like the Couriers (*Cursorius*) they are destitute of the hind toe, and have the same lengthened legs; but the bill is straight instead of curved, and the nostrils furnished with a convex horny membrane, instead of a depressed naked furrow. Little is known of their manners or œconomy. They inhabit (according to Temminck) the confines of sandy deserts, and the arid tracts of Africa and India, appearing to migrate, two species being known to visit the southern parts of Spain. Their flight is said to be amazingly rapid. Nine species have been described with much accuracy by Professor Temminck in his valuable work on the *Gallinaceæ*.

Respecting the generic appellation of these birds, much opposition has been evinced by those authors who have noticed them; for no less than eight different generic names have been proposed; among these the priority, I believe, belongs to Brisson, who distinguished them by the name of *Coturnix* in his *Ornithologia* printed in 1763. This name, therefore, should, I think, have been adopted by modern writers; but as Professor Temminck has given such a full account of the species, and proposed another so very expressive as *Hemipodius*, I think much confusion will be saved by continuing it; particularly as this has already been done by Cuvier; although Dr. Leach, in transcribing Temminck's descriptions, has adopted the unmeaning one of *Turnix* as a *Latin* designation.

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* Communicated by the Author.

HEMIPODIUS NIVOSUS.—White-spotted Turnix.

SPEC. CHAR.—H. supra ferrugineo varius; mento albescente; jugulo pectoreque pallidè ferrugineis, maculis albis nitidis ornatis; corpore albo; uropygio caudæque tectricibus superioribus rufis, immaculatis.

Above varied with ferruginous; chin whitish; throat and breast pale ferruginous with white shining spots; body white; rump and upper tail-covers rufous, unspotted.

DESCRIPTION.—Size of a lark. Total length about five inches. *Bill* dusky, half an inch from the angle of the mouth to the tip; compressed the whole length, but not so much elevated at the base as about the middle, where it slightly rises, and the culmine (or dorsal ridge) forms a gentle curve to the tip, which is not, however, either bent down or notched: the under mandible is straight. *Nostrils* large, placed in a groove, which reaches to more than half the length of the bill, and there terminates in a point; it is defended above by a protuberant, convex, horny covering, which nearly reaches to the end of the groove; the nasal aperture is by a lengthened marginal slit at the base of the protuberance, and close to the margin of the bill. *Upper plumage* reddish brown, striped with whitish fawn colour, and freckled transversely with blackish on the back and scapulary feathers; this whitish colour borders the sides of each feather, and has an internal narrow edging of blackish brown, while the shafts are crossed by short blackish lines: the sides of the head are not spotted, but an obscure whitish band seems to pass both above and below the ears; the back, rump and upper tail-covers are rufous, and likewise unspotted; the lower half of all the wing-covers are white, the upper half ferruginous with white spots and margins. The *Wings* are rounded, the three outer quills progressively longer; the fourth very short, and the fifth surpassing in length all the others *: the lesser quills (nearest the shoulder) are formed like the scapulars and are remarkably long, far exceeding in length all the greater quills: the three external quills are white, with the tips and base black; the remainder, together with the lesser quills, are black with white tips; the *Tail* is nearly hid by its covers, and just exceeds the length of the wings when closed; the feathers are fasciculated, very short, and fawn-coloured, transversely marked with waved bands of dark ferruginous, edged with black. Spurious quills black. Of the *under plumage*, the chin is nearly white, changing gradually to ferruginous; each feather on the neck and breast being spotted with white, which spots possess the singularity of being in some lights almost invisible, but in others of a pure shining snowy whiteness; from the breast to the vent all the plumage is white. *Legs* yellow, one inch and a quarter from the naked part of the thigh to the heel; all the toes are cleft to the base, and the inner one smallest.

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* A singular formation, which I have not the opportunity at present of comparing with other birds of this genus.

The birds of this genus have such a general resemblance in the markings of their variegated plumage, that a minuteness of description, although objectionable on most occasions, here becomes absolutely necessary.

This description was framed from a specimen belonging to Mr. Leadbeater, who received it from Africa; another I have since seen in the well-known Museum of Mr. Brookes.

LXXI. *A Letter from* JOHN POND, *Esq. Astronomer Royal, to Sir* HUMPHRY DAVY, *Bart. President of the Royal Society, relative to a Derangement in the Mural Circle at the Royal Observatory**.

MY DEAR SIR, — THE interest which the Royal Society has always taken in every thing relating to this Observatory, and to which may be attributed its present prosperous condition, will, I trust, render unnecessary any apology for this communication.

I wish to make known to astronomers, as soon as possible, a derangement that has for some time past existed in the mural circle, and of which I have not, till lately, been able to ascertain the cause with certainty.

This derangement began, I believe, about the autumn of the year 1819; the position of the telescope was then changed; and from that time the error has been gradually increasing till last summer, when the cause was distinctly ascertained, and the proper remedy applied.

In the Preface to the Greenwich Observations for the year 1820, now printing, I shall have an opportunity of stating the amount of this error, and the correction which should be applied to the observations made within the two last years. At present it will be sufficient to mention, in as few words as possible, the cause of this error.

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* From the Philosophical Transactions, Part I. for 1822.

Those who are acquainted with the construction of the Greenwich Mural Circle, are aware, that though the telescope may be applied to every part of the circle, yet, when fixed for observation, the principle of the instrument requires that the tube, especially at its extremities, should be so firmly fixed to the circle as to form one piece with it: to accomplish this, connecting braces are attached at each end of the telescope. It now appears that these braces have, in progres of time, become insecure, owing to the screws which fastened them having given way. The effect of this will be, to permit the ends to bend from the centre instead of retaining, as they ought to do, an invariable position with respect to the circle. Under these circumstances, when the telescope is directed to the zenith, the position may be considered as free from error; but when the instrument is moved either towards the north or south horizon, should either extremity bend more than the other, an error will take place, and will increase from the zenith towards the horizon, but in what exact proportion, remains to be determined by future observations.

The cause of this error being thus ascertained, Mr. Troughton has applied additional braces to connect the telescope with the circle, sufficiently strong, I should conceive, to prevent the possibility of such an accident for the future.

This alteration has already produced such an improvement in the observations, as prove sufficiently that the source of error has not been mistaken. Of the published Observations, only those made in the three last months of the year 1819 are affected by this error, and that in so very small a degree, as must have entirely escaped notice, had it not afterwards increased.

During the year 1820 the error increased; but did not, I believe, in the distance from the pole to the equator amount to two seconds; at altitudes lower than Sirius, and at the altitude of the sun at the winter solstice, the error may have been greater than two seconds, but did not exceed four.

But after the month of February 1821, the error rapidly increased; and this ultimately led to a discovery of the cause.

My present object in this letter, is simply to state these circumstances to the Society. I shall defer a more detailed account of the nature of this derangement, and of its effects, till I shall be enabled, by a sufficient number of observations made with the instrument in its improved state, to ascertain, with some degree of certainty, at what period the derangement took place, and what corrections are required to be applied, till the instrument was restored to its perfect state.

I have the honour to be, my dear sir,
With the highest regard,
Your most obedient humble servant,

Royal Observatory, Nov. 21, 1821. JOHN POND.

LXXII. *On the Subsidence of Aqueous Vapour; and its Repose on the Beds of Rivers.* By JOHN MURRAY, *F.L.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—THE President of the Royal Society has ingeniously ascribed the formation of mists in particular situations, to the superior temperature maintained by currents of water from below, these supplanting the superficial wave and supplying its place, when cooled down by radiation. This movement and interchange would continue so long as any part of the mass remained above 45°. On this principle the air incumbent on the bosom of the river would, having its loss by radiation thus compensated for, remain at a higher temperature than that reposing on the banks of the river which can have no such compensation. The cooler atmosphere of the latter would therefore move towards the former, and mingling with it, *mist* would be formed.

Now, though the currency of a rapid river, as the Rhone for instance, might, *prima facie*, seem to disturb the rise and fall from below and above, still it must be confessed that the view sustained, is every way worthy of its distinguished author.

My own *experiments* on the Seine and the Po seemed not to harmonize with these views; but the thermometer might be in fault, and I was willing to cede the point.

On the 3d instant a fine opportunity was afforded me for ascertaining the question with exactness and precision. The numerous repetitions of the experiments, and the great extent of surface examined thermometrically, with the extreme delicacy of the instrument, enable me to submit the results to you with unusual confidence.

Rain obtained in the early part of the day, and the sky was dark and lowering. The horizontality of the clouds, with their parallelism in the distant sky, seemed to indicate the prevalence of electric influence. Toward the afternoon, mist formed over the bed of the Severn and settled on its surface. Three different barometers were examined on the occasion. One, thirty feet above the Severn, stood at 29·5 inches, surface somewhat concave; another, nine feet above the level of the river, indicated the altitude of the mercury to be 29·4 inches; and on the same plane a wheel barometer was stationary at "changeable."

Having taken a boat I was enabled to ascertain the temperature of the river for some miles, both toward the centre and toward the margin. From four to six o'clock P.M. the *water* was uniform in all its extent, at least the range was within the limits 52° to 52·5 Fahrenheit,—the ball of the thermometer which projected two inches from the scale afforded me the measure of immersion. The atmosphere resting on the river at *an inch from its surface* sustained a temperature which fluctuated between the points 57·5 and 58·25 Fahr.

The atmosphere over the banks of the Severn was examined on *both* sides of the river, and its range included 61° to 62° of the scale of Fahr.

The fog or mist which had been entirely limited to the bed of the river, became general at night, and the nocturnal sky was without a cloud.

I tender you the facts, and have no theory to support. The evolution and subsidence of the aqueous vapour, may *in this instance*, at least, be perhaps explained by supposing that a portion of the cooler air from over the river moves to that of superior temperature resting on the river bank, and the former being thus *attenuated in density*, the deposition of aqueous vapour ensues as a necessary consequence.

I have the honour to be,
Gentlemen,
Your most obedient servant,

Bewdley, October 5, 1822. J. MURRAY.

LXXIII. *Notice of some New Galvanic Experiments and Phænomena.* By JOHN MURRAY, *F.L.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—OUR views on the nature of the agencies developed through the medium of the galvanic instrument, and their relations to the phænomena of heat and magnetism, are exceedingly perplexed and obscure. We have many valuable facts in our possession, but their source and connexion are utterly unknown. We shall surely do well to swell the great volume of facts, and wait with patience the revelation to which they point, and which will form an epocha as rich and brilliant as any of those which have blessed the annals of science. This may be reserved for the NEWTON of a future age; but we shall have merit in accumulating the materials that are finally to unfold in harmony and beauty beneath the plastic touch of that great genius, which may be yet to come.

It is in this spirit that I submit to you a variety of galvanic experiments, as far as *I* know, *new*; and not unaccompanied, it is presumed, with interest.

I find that silver foil may be deflagrated, and in a very brilliant manner, in *alcohol* and in *ether*, when immersed in these liquids, and the wires proceeding from the ends of the porcelain cells are introduced and brought into contact with the interposed silver foil. In like manner may gold and copper foil, &c. be so ignited.

The same brilliant deflagration may be sustained in *naphtha*, *sulphuret of carbon*, and the *vapour of ether*.

No remarkable brilliancy or other phænomenon attended the burning of the silver-leaf, &c. in *oxygen* or *chlorine*.

The deflagration of silver-leaf in *carbonic acid gas* yields a bluish green light.

Silver-leaf was deflagrated with great beauty and brilliancy in an inverted jar of *hydrogen*; and at the close of the experiment the wires were withdrawn to ignite the gas at the orifice, and to prove that the contained elastic medium had suffered no change.

Alcohol being kindled in a watch-glass; a steel wire was stretched across the flame. This wire was ignited and expanded *only* where it passed through the outer laminæ, on each side of the cone*; but on the conducting rods being attached, the ignition *darted through the wire*, though the portion of wire *within* the body of flame did *not expand*.—At this moment the wire was fused and dispersed in brilliant ignited globules.

A *platinum wire* similarly circumstanced, expanded, and was ignited at the *outer edge* or fringe of the conical film of flame. So soon as the communication was made with the conductors, the ignition, as before, shot through the wire and across the cone. In that portion of the wire which was encased by the flame, no expansion of volume appeared. Fusion of the wire supervened.

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* This serves to prove in like manner, if indeed any further evidence were necessary, that *flame* is merely *superficial*. At page 27 of Mr. Children's truly valuable translation of "Berzelius on the Blowpipe," the discovery of the *hollowness* of flame is *exclusively* attributed to Mr. Oswald Sym. But *I had also*, Mr. Children will find, announced the same fact *simultaneously*, as recorded in the *contemporaneous* Number of the Philosophical Magazine. If there was any thing in the asserted *cooling* influence of *wire-gauze*, Mr. Sym's experiment was *no proof whatever*. My experiment was made with a *plate of glass*, and was clear and unequivocal, I afterwards proved the fact in the most *incontestible manner*, by showing that when an *ignited taper* was introduced into such a cone it became instantly *extinct*, and, moreover, even that *phosphorus* itself, in the most rapid

The phænomena here exhibited clearly show that the heat of the ambient medium *within* the cone is less intense than that displayed at the edge or superficial envelope of flame.

Plumbago finely acuminated and attached to one of the conductors was introduced into the cone of flame; when the contact was completed with the opposite pole, a very *brilliant star-like combustion* supervened.

All these phænomena seem to show that the deflagration of metallic laminæ and ignition of wires are perfectly *independent of the medium* in which such are posited; and also that the *heat of common flame* conjoined with that evolved through the agency of galvanism do produce by their *combined agency* an aggregate of superior intensity in relation to ignition.

Part of a steel wire was *wrapped round with platinum wire.* On completion of the contact, *only* that part of the steel wire *denuded of the platinum* could be *ignited*, the remaining part not being so susceptible.

This would tend to prove the *materiality* of caloric. *Two* conducting wires instead of one being employed, a channel of double diameter was provided (though by separate canals) for the flux of the calorific matter from pole to pole.

If the ends of the conductors repose on a narrow *magnetized* plate of steel, an interposed thread of steel or platinum is *incapable of being ignited:* the directors may be even made to glide along the wire until contact is complete; yet ignition will not follow. So soon, however, as either end is elevated from the bar, the wire is ignited and fused.

If a steel wire be interposed between the directors, and the distance be sufficient, so as to elevate it to a temperature not *exceeding a cherry red heat*, the wire becomes blued in patches,

rapid combustion, is as completely extinguished as if plunged into water. Potassium itself cannot by possibility be made to exhibit *flame* under such circumstances. A beautiful expression of the structure of flame may be obtained by introducing a *straw* transversely, the outer or *exterior surface* of the flame will *char* and *consume the portion which so protrudes*, but the *intermediate* part will remain untouched by the flame; and over it, the fire has no power. The distinguished chemist in question will readily recollect his pointing out to me that gunpowder might be ignited by being let fall *through the exterior* part of flame; and when Mr. Pepys was, *last winter*, told of my experiments of the *nonsupport of combustion within flame*, he expressed surprise it should be so.

and at the same time becoming *magnetic*, cannot afterwards be ignited at distances however limited.

These latter facts show clearly the reason of the *anomalous ignition of steel wire*; or, in other words, it explains the phenomenon—that though it may ignite in the FIRST instance, we may nevertheless not be able to accomplish it in *subsequent* attempts; for the wire in becoming *magnetic*, becomes also a *superior conductor of the caloric* superinduced by the galvanic action.

I have the honour to be, gentlemen,
Your most obedient and very humble servant,
Birmingham, Oct. 8, 1822. J. MURRAY.

LXXIV. *On the North Polar Distances of the principal fixed Stars, deduced from the Observations made at the Royal Observatory at Greenwich.*

OUR readers may recollect that the catalogue of these stars, inserted in the Nautical Almanac for 1824, differed in North Polar Distance so much from the preceding catalogues as to lead to a conclusion that there was some error in the observations: and which was, in a measure, confirmed by a letter of the Astronomer Royal, which was read before the Royal Society last year, relative to a derangement in the Mural Circle*. Those differences amounted, in the case of *Procyon*, to upwards of 9″; and, in the case of *Sirius*, to nearly 7″. The catalogue just published, in the Nautical Almanac for 1825, which is deduced from observations made with the Mural Circle *since* September 1821, does not wholly remove those doubts: as it still appears to vary very considerably from *former* obervations. In fact, there seems to be but little difference between the catalogue published in the Nautical Almanac for 1824, and that now published in the Nautical Almanac for 1825; as will be seen by the following comparison. And, where the difference is *now* the greatest (as is the case with γ *Ursæ Majoris*), the *former* observations almost uniformly *agreed* with each other. We have too much confidence in the excellence and perfection of the Mural Circle, and in the acknowledged accuracy of the Astronomer Royal, to believe that these anomalies will long remain unreconciled or unexplained.

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* See this Letter in page 355 of our present Number.

Mean North Polar Distances of the principal Stars on January 1, 1820.

No.	Names of Stars.	N. P. D.	Naut. Alman.		Differen.
			1824	1825	
		° ′	″	″	″
1	γ Pegasi	75. 49.	0	0	0
2	α Cassiopeiæ ...	34. 27.	4	5	+ 1
3	Polaris	1. 39.	5,5	5,5	0
4	α Arietis	67. 23.	37	36	− 1
5	α Ceti	86. 37.	18	20	+ 2
6	α Persei	40. 47.	18	19	+ 1
7	Aldebaran ...	73. 51.	43	41	− 2
8	Capella	44. 11.	51	51	0
9	Rigel	98. 25.	0	2	+ 2
10	β Tauri	61. 33.	19	18	− 1
11	α Orionis	82. 38.	8	8	0
12	Sirius	106. 28.	36	35	− 1
13	Castor	57. 43.	39	38	− 1
14	Procyon	84. 19.	19	18	− 1
15	Pollux	61. 32.	55	53	− 2
16	α Hydræ	97. 52.	57	58	− 1
17	Regulus	77. 9.	24	24	0
18	α Ursæ Maj. ...	27. 16.	45	46	+ 1
19	β Leonis	74. 25.	18	18	0
20	β Virginis	87. 13.	15	15	0
21	γ Ursæ Maj. ...	35. 18.	19	15	− 4
22	Spica	100. 13.	3	4	+ 1
23	η Ursæ Maj. ...	39. 47.	4	5	+ 1
24	Arcturus ...	69. 52.	33	32	− 1
25	α¹ Libræ	105. 14.	26	29	+ 3
26	α² ———	105. 17.	10	10	0
27	β Ursæ Min. ...	15. 6.	31	31	0
28	α Coronæ Bor....	62. 40.	23	23	0
29	α Serpentis ...	83. 0.	3	2	− 1
30	Antares	116. 1.	19	18	− 1
31	α Herculis... ...	75. 23.	47	46	− 1
32	α Ophiuchi ...	77. 18.	2	1	− 1
33	γ Draconis ...	38. 29.	9	9	0
34	α Lyræ	51. 22.	40	40	0
35	γ Aquilæ	79. 49.	2	2	0
36	α ———	81. 35.	57	56	− 1
37	β ———	84. 2.	4	4	0
38	α¹ Capricorni ...	103. 3.	21	20	− 1
39	α² ——————— ...	103. 5.	37	37	0
40	α Cygni	45. 21.	30	30	0
41	α Cephei	28. 10.	27	28	+ 1
42	β ———————	20. 13.	40	40	0
43	α Aquarii	91. 11.	21	22	+ 1
44	Fomalhaut ...	120. 34.	26	24	− 2
45	α Pegasi	75. 45.	39	40	+ 1
46	α Andromedæ ...	61. 54.	13	12	− 1

LXXV. *On*

LXXVI.—*On the Alloys of Steel.* *By* J. Stodart, *Esq. F.R.S. and Mr.* M. Faraday, *Chemical Assistant in the Royal Institution**.

The alloys of steel made on a small scale in the laboratory of the Royal Institution proving to be good, and the experiments having excited a very considerable degree of interest both at home and abroad, gave encouragement to attempt the work on a more extended scale; and we have now the pleasure of stating, that alloys similar to those made in the Royal Institution, have been made for the purpose of manufacture; and that they prove to be, in point of excellence, in every respect equal, if not superior, to the smaller productions of the laboratory. Previous, however, to extending the work, the former experiments were carefully repeated, and to the results were added some new combinations, namely, steel with palladium, steel with iridium and osmium, and latterly, steel with chromium. In this last series of experiments we were particularly fortunate, having by practice acquired considerable address in the management of the furnaces, and succeeded in procuring the best fuel for the purpose. Notwithstanding the many advantages met with in the laboratory of the Royal Institution, the experiments were frequently rendered tedious from causes often unexpected, and sometimes difficult to overcome; among these, the failure of crucibles was perhaps the most perplexing. We have never yet found a crucible capable of bearing the high degree of temperature required to produce the perfect reduction of titanium; indeed we are rather disposed to question whether this metal has ever been so reduced: our furnaces are equal † (if any are) to produce this effect, but hitherto we have failed in procuring a crucible.

The metals that form the most valuable alloys with steel are silver, platina, rhodium, iridium and osmium, and palladium; all of these have now been made in the large way, except indeed the last named. Palladium has, for very obvious reasons, been used but sparingly; four pounds of steel with $\frac{1}{100}$ part of palladium, has however been fused at once, and the compound is truly valuable, more especially for making instruments that require perfect smoothness of edge.

We are happy to acknowledge the obligations due from us to Dr. Wollaston, whose assistance we experienced in every stage of our progress, and by whom we were furnished with all the scarce and valuable metals; and that with a liberality

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* From the Philosophical Transactions, Part II. for 1822.

† We have succeeded in fusing in these furnaces rhodium, and also, though imperfectly, platinum in crucibles.

which enabled us to transfer our operations from the laboratory of the chemist, to the furnace of the maker of cast steel.

In making the alloys on a large scale, we were under the necessity of removing our operations from London to a steel furnace at Sheffield; and being prevented by other avocations from giving personal attendance, the superintendence of the work was consequently intrusted to an intelligent and confidential agent. To him the steel, together with the alloying metals in the exact proportion, and in the most favourable state for the purpose, was forwarded, with instructions to see the whole of the metals, and nothing else, packed into the crucible, and placed in the furnace, to attend to it while there, and to suffer it to remain for some considerable time in a state of thin fusion, previous to its being poured out into the mould. The cast ingot was next, under the same superintendence, taken to the tilting mill, where it was forged into bars of a convenient size, at a temperature not higher than just to render the metal sufficiently malleable under the tilt hammer. When returned to us, it was subjected to examination both mechanical and chemical, as well as compared with the similar products of the laboratory. From the external appearance, as well as from the texture of the part when broken by the blow of the hammer, we were able to form a tolerably correct judgement as to its general merits; the hardness, toughness, and other properties, were farther proved by severe trials, after being fashioned into some instrument, or tool, and properly hardened and tempered.

It would prove tedious to enter into a detail of experiments made in the Royal Institution; a brief notice of them will at present be sufficient. After making imitations of various specimens of meteoric iron, by fusing together pure iron and nickel, in proportions of 3 to 10 per cent., we attempted making an alloy of steel with silver, but failed, owing to a superabundance of the latter metal; it was found, after very many trials, that only the $\frac{1}{500}$ part of silver would combine with steel, and when more was used a part of the silver was found in the form of metallic dew, lining the top and sides of the crucible; the fused button itself was a mere mechanical mixture of the two metals, globules of silver being pressed out of the mass by contraction in cooling, and more of these globules being forced out by the hammer in forging; and further, when the forged piece was examined, by dissecting it with diluted sulphuric acid, threads or fibres of silver were seen mixed with the steel, having something of the appearance of steel and platina when united by welding: but when the proportion of silver was only $\frac{1}{500}$ part, neither dew, globules,

bules, nor fibres appeared, the metals being in a state of perfect chemical combination, and the silver could only be detected by a delicate chemical test.

With platina and rhodium, steel combines in every proportion; and this appears also to be the case with iridium and osmium: from 1 to 80 per cent. of platina was perfectly combined with steel in buttons of from 500 to 2000 grains. With rhodium, from 1 to 50 per cent. was successfully used. Equal parts by weight of steel and rhodium gave a button, which, when polished, exhibited a surface of the most exquisite beauty: the colour of this specimen is the finest imaginable for a metallic mirror, nor does it tarnish by long exposure to the atmosphere: the specific gravity of this beautiful compound is 9.176. The same proportion of steel and platina gave a good button, but a surface highly crystalline renders it altogether unfit for a mirror. In the laboratory we ascertained that, with the exception of silver, the best proportion of the alloying metal, when the object in view was the improvement of edge-tools, was about $\frac{1}{100}$ part, and in this proportion they have been used in the large way. It may be right to notice, that in fusing the metals in the laboratory no flux whatever was used, nor did the use of any ever appear to be required.

Silver being comparatively of little value with some of the alloying metals, we were disposed to make trial with it as the first experiment in the large way. 8lbs. of very good Indian steel was sent to our agent, and with it $\frac{1}{500}$ part of pure silver: a part of this was lost owing to a defect in the mould; a sufficient quantity was however saved, to satisfy us as to the success of the experiment. This, when returned, had the most favourable appearance both as to surface and fracture; it was harder than the best cast steel, or even than the Indian wootz, with no disposition whatever to crack, either under the hammer, or in hardening. Some articles, for various uses, have been made from this alloy; they prove to be of a very superior quality; its application will probably be extended not only to the manufacture of cutlery, but also to various descriptions of tools; the trifling addition of price cannot operate against its very general introduction. The silver alloy may be advantageously used for almost every purpose for which good steel is required.

Our next experiment made in the large way, was with steel and platina. 10lbs. of the same steel, with $\frac{1}{100}$ part of platina, the latter in the state produced by heating the ammonia muriate in a crucible to redness, was forwarded to our agent, with instructions to treat this in the same way as the last-named metals. The whole of this was returned in bars remarkable for smoothness of surface and beauty of fracture. Our own observation, as well as that of the workmen employed to make from it various articles of cutlery, was, that this alloy, though not so hard as the former, had considerably more toughness: this property will render it valuable for every purpose where tenacity, as well as hardness, is required; neither will the expense of platina exclude it from a pretty general application in the arts; its excellence will much more than repay the extra cost.

The alloys of steel with rhodium have also been made in the large way, and are perhaps the most valuable of all; but these, however desirable, can never, owing to the scarcity of the metal, be brought into very general use. The compound of steel, iridium, and osmium, made in the large way, is also of great value; but the same cause, namely, the scarcity and difficulty of procuring the metals, will operate against its very general introduction. A sufficient quantity of these metals may, perhaps, be obtained to combine with steel for the purpose of making some delicate instruments, and also as an article of luxury, when manufactured into razors. In the mean time, we have been enabled, repeatedly, to make all these alloys (that with palladium excepted) in masses of from 8 to 20lbs. each; with such liberality were we furnished with the metals from the source already named.

A point of great importance in experiments of this kind was, to ascertain whether the products obtained were exactly such as we wished to produce. For this purpose, a part of each product was analysed, and in some cases the quantity ascertained; but it was not considered necessary in every case to verify the quantity by analysis, because, in all the experiments made in the laboratory, the button produced after fusion was weighed, and if it fell short of the weight of both metals put into the crucible, it was rejected as imperfect, and put aside. When the button gave the weight, and on analysis gave proofs of containing the metal put in to form the alloy, and also on being forged into a bar and acted on by acids, presented an uniform surface, we considered the evidence of its composition as sufficiently satisfactory. The processes of analysis, though simple, we shall briefly state: the information may be desirable to others who may be engaged on similar experiments; and further, may enable every one to detect any attempt at imposition. It would be very desirable at present, to possess a test as simple, by which we could distinguish the wootz, or steel of India, from that of Europe; but this, unfortunately, requires a much more difficult process of analysis.

To ascertain if platina is in combination with steel, a small portion of the metal, or some filings taken from the bar, is to be put into dilute sulphuric acid; there will be rapid action; the iron will be dissolved, and a black sediment left, which will contain carbon, hydrogen, iron, and platina; the carbon and hydrogen are to be burnt off, the small portion of iron separated by muriatic acid, and the residuum dissolved in a drop or two of nitro-muriatic acid. If a piece of glass be moistened with this solution, and then heated by a spirit-lamp and the blow-pipe, the platina is reduced, and forms a metallic coating on the glass.

In analysing the alloy of steel and silver, it is to be acted on by dilute sulphuric acid, and the powder boiled in the acid; the silver will remain in such a minute state of division, that it will require some time to deposit. The powder is then to be boiled in a small portion of strong muriatic acid *; this will dissolve the iron and silver, and the latter will fall down as a chloride of silver on dilution with water; or the powder may be dissolved in pure nitric acid, and tested by muriatic acid and ammonia.

The alloy of steel and palladium, acted on by dilute sulphuric acid, and boiled in that acid, left a powder, which, when the charcoal was burnt from it, and the iron partly separated by cold muriatic acid, gave on solution in hot muriatic acid, or in nitro-muriatic acid, a muriate of palladium; the solution, when precipitated by prussiate of mercury, gave prussiate of palladium; and a glass plate moistened with it and heated to redness, became coated with metallic palladium.

The residuum of the rhodium alloy obtained by boiling in diluted sulphuric acid, had the combustible matter burnt off, and the powder digested in hot muriatic acid: this removed the iron; and by long digestion in nitro-muriatic acid, a muriate of rhodium was formed, distinguishable by its colour, and by the triple salt it formed with muriate of soda.

To analyse the compound of steel with iridium and osmium, the alloy should be acted on by dilute sulphuric acid, and the residuum boiled in the acid; the powder left is to be collected and heated with caustic soda in a silver crucible to dull redness for a quarter of an hour, the whole to be mixed with water, and having had excess of sulphuric acid added, it is to be distilled, and that which passes over condensed in a flask: it will be a solution of oxide of osmium, will have the peculiar smell belonging to that substance, and will give a blue precipitate

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* Although it is a generally received opinion that muriatic acid does not act on silver, yet that is not the case; pure muriatic acid dissolves a small portion of silver very readily.

pitate with tincture of galls. The portion in the retort being then poured out, the insoluble part is to be washed in repeated portions of water, and then being first slightly acted on by muriatic acid to remove the iron, is to be treated with nitro-muriatic acid, which will give a muriate of iridium.

In these analyses, an experienced eye will frequently perceive, on the first action of the acid, the presence of the alloying metal. When this is platina, gold, or silver, a film of the metal is quickly formed on the surface of the acid.

Of alloys of platina, palladium, rhodium, and iridium and osmium, a ready test is offered when the point is not to ascertain what the metal is, but merely whether it be present or not. For this purpose we have only to compare the action of the same acid on the alloy and on a piece of steel; the increased action on the alloy immediately indicates the presence of the metal; and by the difference of action, which on experience is found to be produced with the different metals, a judgement may be formed even of the particular one present.

The order in which the different alloys stand with regard to this action, is as follows: steel, chromium alloy, silver alloy, gold alloy, nickel alloy, rhodium alloy, iridium and osmium alloy, palladium alloy, platina alloy. With similar acid the action on the pure steel was scarcely perceptible; the silver alloy gave very little gas, nor was the gold much acted on. All the others gave gas copiously, but the platina alloy in most abundance.

In connection with the analysis of these alloys, there are some very interesting facts to be observed during the action of acids on them, and perhaps none of these are more striking than those last referred to. When the alloys are immersed in diluted acid, the peculiar properties which some of them exhibit, not only mark and distinguish them from common steel, and from each other, but also give rise to some considerations on the state of particles of matter of different kinds when in intimate mixture or in combination, which may lead to clearer and more perfect ideas on this subject.

If two pieces, one of steel, and one steel alloyed with platina, be immersed in weak sulphuric acid, the alloy will be immediately acted on with great rapidity and the evolution of much gas, and will shortly be dissolved, whilst the steel will be scarcely at all affected. In this case, it is hardly possible to compare the strength of the two actions. If the gas be collected from the alloy and from the steel for equal intervals of time, the first portions will surpass the second some hundreds of times.

A very small quantity of platina alloyed with steel confers this property on it: $\frac{1}{400}$ increased the action considerably; with $\frac{1}{200}$ and $\frac{1}{100}$ it was powerful; with 10 per cent. of platina it acted, but not with much power; with 50 per cent. the action was not more than with steel alone; and an alloy of 90 platina with 20 steel was not affected by the acid.

The action of other acids on these alloys is similar to that of sulphuric acid, and is such as would be anticipated: dilute muriatic acid, phosphoric acid, and even oxalic acid, acted on the platina alloy with the liberation of more gas than from zinc; and tartaric acid and acetic acid rapidly dissolved it. In this way chalybeate solutions, containing small portions of protoxide of iron, may be readily obtained.

The cause of the increased action of acids on this and similar alloys, is, as the President of this Society suggested to us, probably electrical. It may be considered as occasioned by the alloying metal existing in such a state in the mass, that its particles form voltaic combinations with the particles of steel, either directly or by producing a definite alloy, which is diffused through the rest of the steel; in which case the whole mass would be a series of such voltaic combinations: or it may be occasioned by the liberation, on the first action of the acid, of particles which, if not pure platina, contain, as has been shown, a very large proportion of that metal, and which, being in close contact with the rest of the mass, form voltaic combinations with it in a very active state: or, in the third place, it may result from the iron being mechanically divided by the platina, so that its particles are more readily attacked by the acid, analogous to the case of proto-sulphuret of iron.

Although we have not been able to prove by such experiments, as may be considered strictly decisive, to which of these causes the action is owing, or how much is due to any of them, yet we do not hesitate to consider the second as almost entirely, if not quite, the one that is active. The reasons which induce us to suppose this to be the true cause of the action, rather than any peculiar and previous arrangement of the particles of steel and platina, or than the state of division of the steel, are, that the two metals combine in every proportion we have tried, and do not, in any case, exhibit evidences of a separation between them, like those, for instance, which steel and silver exhibit; that when, instead of an acid, weaker agents are used, the alloy does not seem to act with them as if it was a series of infinitely minute voltaic combinations of steel and platina, but exactly as steel alone would do; that the mass does not render platina wire more negative than steel, as it probably in the third case would do; that it does not rust more rapidly in a damp atmosphere; and that when placed in saline solutions, as muriate of soda, &c., there is no action takes place between them. In such cases it acts just like steel; and no agent that we have as yet tried, has produced voltaic action that was not first able to set a portion of the platina free by dissolving out the iron.

Other interesting phenomena exhibited by the action of acid on these steels, are the differences produced when they are hard and when soft. Mr. Daniel, in his interesting paper on the mechanical structure of iron, published in the Journal of Science, has remarked, that pieces of hard and soft steel being placed in muriatic acid, the first required five-fold the time of the latter to saturate the acid; and that when its surface was examined, it was covered with small cavities like worm-eaten wood, and was compact and not at all striated, and that the latter presented a fibrous and wavy texture.

The properties of the platina alloy have enabled us to observe other differences between hard and soft steel equally striking. When two portions of the platina alloy, one hard and one soft, are put into the same diluted sulphuric acid and suffered to remain for a few hours, then taken out and examined, the hard piece presents a covering of a metallic black carbonaceous powder, and the surface is generally slightly fibrous; but the soft piece, on examination, is found to be covered with a thick coat of grey metallic plumbaginous matter, soft to the touch, and which may be cut with a knife, and its quantity seven or eight times that of the powder on the hard piece: it does not appear as if it contained any free charcoal, but considerably resembles the plumbaginous powder Mr. Daniel describes as obtained by the action of acid on cast iron.

The same difference is observed if pure steel be used, but it is not so striking; because, being much less rapidly attacked by the acid, it has to remain longer in it, and the powder produced is still further acted on.

The powder procured from the soft steel or alloy in these experiments, when it has not remained long in the acid, exactly resembles finely divided plumbago, and appears to be a carburet of iron, and probably of the alloying metal also. It is not acted on by water, but in the air the iron oxidates and discolours the substance. When it remains long in the acid, or is boiled in it, it is reduced to the same state as the powder from the hard steel or alloy.

When any of these residua are boiled in diluted sulphuric or muriatic acid, protoxide of iron is dissolved, and a black powder remains unalterable by the further action of the acid; it is apparently in greater quantity from the alloys than from pure steel, and when washed, dried, and heated to 300° or 400° in the air, burns like pyrophorus, with much fume; or if lighted, burns like bitumen, and with a bright flame; the residuum is protoxide of iron, and the alloying metal. Hence, during the action of the acid on the steel, a portion of hydrogen enters into combination with part of the metal and the charcoal, and forms an inflammable compound not acted upon by the acid.

Some striking effects are produced by the action of nitric acid on these powders. If that from pure steel be taken, it is entirely dissolved; and such is also the case if the powder be taken from an alloy, the metal of which is soluble in nitric acid; but if the powder is from an alloy, the metal of which is not soluble in nitric acid, then a black residuum is left not touched by the acid; and which, when washed and carefully dried, is found, when heated, to be deflagrating; and with some of the metals, when carefully prepared, strongly explosive.

The fulminating preparation obtained from the platina alloy, when dissolved in nitro-muriatic acid, gave a solution containing much platina, and very little iron. When a little of it was wrapped in foil and heated, it exploded with much force, tearing open the foil, and evolving a faint light. When dropped on the surface of heated mercury, it exploded readily at 400° of Fahrenheit, but with difficulty at 370°. When its temperature was raised slowly, it did not explode, but was decomposed quietly. When detonated in the bottom of a hot glass tube, much water and fume were given off, and the residuum collected was metallic platina with a very little iron and charcoal. We are uncertain how far this preparation resembles the fulminating platina of Mr. Edmund Davy.

In these alloys of steel the differences of specific gravity are not great, and may probably be in part referred to the denser state of the metals from more or less hammering: at the same time it may be observed, that they are nearly in the order of the specific gravities of the respective alloying metals.

The alloys of steel with gold, tin, copper, and chromium, we have not attempted in the large way. In the laboratory, steel and gold were combined in various proportions;—none of the results were so promising as the alloys already named, nor did either tin or copper, as far as we could judge, at all improve steel. With titanium we failed, owing to the imperfection of crucibles. In one instance, in which the fused button gave a fine damask surface, we were disposed to attribute the appearance to the presence of titanium; but in this we were mistaken;—the fact was, we had unintentionally made wootz. The button, by analysis, gave a little silex and alumine, but not an atom of titanium; menachanite, in a particular state of preparation, was used: this might possibly contain the earths or their basis, or they may have formed a part of the crucible.

M. Berthier,

M. Berthier, who first made the alloy of steel and chromium*, speaks very favourably of it. We have made only two experiments. 1600 grains of steel, with 16 of pure chrome, were packed into one of the best crucibles, and placed in an excellent blast furnace: the metals were fused, and kept in that state for some time. The fused button proved good and forged well: although hard, it showed no disposition to crack. The surface being brightened, and slightly acted on by dilute sulphuric acid, exhibited a crystalline appearance; the crystals, being elongated by forging, and the surface again polished, gave, by dilute acid, a very beautiful damask. Again, 1600 grains of steel with 48 of pure chrome were fused: this gave a button considerably harder than the former. This too was as malleable as pure iron, and also gave a very fine damask. Here a phenomenon rather curious was observed: the damask was removed by polishing, and restored by heat without the use of any acid. The damasked surface, now coloured by oxidation, had a very novel appearance: the beauty was heightened by heating the metal in a way to exhibit all the colours caused by oxidation, from pale straw to blue, or from about 430 to 600° of Fahrenheit. The blade of a sabre, or some such instrument, made from this alloy, and treated in this way, would assuredly be beautiful, whatever its other properties might be; for of the value of the chrome alloy for edge tools we are not prepared to speak, not having made trial of its cutting powers. The sabre blade, thus coloured, would amount to a proof of its being well tempered; the blue back would indicate the temper of a watch spring, while the straw colour towards the edge would announce the requisite degree of hardness. It is confessed, that the operation of tempering any blade of considerable length in this way, would be attended with some difficulty.

In the account now given of the different alloys, only one triple compound is noticed, namely, steel, iridium and osmium; but this part of the subject certainly merits further investigation, offering a wide and interesting field of research. Some attempts to form other combinations of this description proved encouraging, but we were prevented, at the time, by various other avocations, from bestowing on them that attention and labour they seemed so well to deserve †.

It is a curious fact, that when pure iron is substituted for steel, the alloys so formed are much less subject to oxidation. 3 per cent. of iridium and osmium fused with some pure iron, gave a button, which when forged and polished was exposed, with many other pieces of iron, steel, and alloys, to a moist atmosphere: it was the last of all showing any rust. The colour of this compound was distinctly blue; it had the property of becoming harder when heated to redness, and quenched in a cold fluid. On observing this steel-like character, we suspected the presence of carbon: none, however, was found, although carefully looked for. It is not improbable that there may be other bodies, besides charcoal, capable of giving to iron the properties of steel; and though we cannot agree with M. Boussingault *, when he would replace carbon in steel by silica or its base, we think his experiments very interesting on this point, which is worthy further examination.

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* *Annales de Chimie*, xvii. 55.

† It is our intention to continue these experiments at every opportunity; but they are laborious, and require much time and patience.

We are not informed as to what extent these alloys, or any of them, have been made at home, or to what uses they have been applied; their more general introduction in the manufacture of cutlery would assuredly add to the value, and consequently to the extension of that branch of trade. There are various other important uses to which the alloys of steel may advantageously be applied. If our information be correct, the alloy of silver, as well as that of platina, has been to some considerable extent in use at His Majesty's Mint. We do know, that several of the alloys have been diligently and successfully made on the Continent, very good specimens of some of them having been handed to us; and we are proud of these testimonies of the utility of our endeavours.

To succeed in making and extending the application of these new compounds, a considerable degree of faithful and diligent attention will be required on the part of the operators. The purity of the metals intended to form the compound is essential; the perfect and complete fusion of both must, in every case, be ascertained: it is further requisite, that the metals be kept for some considerable time in the state of thin fusion: after casting, the forging is with equal care to be attended to; the metal must on no account be overheated; and this is more particularly to be attended to when the alloying metal is fusible at a low temperature, as silver. The same care is to be observed in hardening: the article is to be brought to a cherry-red colour, and then instantly quenched in the cold fluid.

In tempering, which is best performed in a metallic bath properly constructed, the bath will require to be heated for the respective alloys, from about 70° to 100° of Fahrenheit above the point of temperature required for the best cast steel. We would

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* *Annales de Chimie*, xvi. 1.

would further recommend, that this act of tempering be performed twice; that is, at the usual time before grinding, and again just before the last polish is given to the blade. This second tempering may perhaps appear superfluous, but upon trial its utility will be readily admitted. We were led to adopt the practice by analogy, when considering the process of making and tempering watch-springs.

LXXVII. *On Mr.* J. MURRAY'S *Communications. By* J. MOORE, *Junior.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—MR. Murray does not seem to like notes of interrogation; yet the very first sentence of his communication to you in your last September Number, requires I should ask him a question. I should not have noticed this sentence, but I suppose Mr. Murray has a motive in using the word *repeat.* He says: "I repeat that I see no necessity whatever for republishing on the part of Mr. John Moore, Jun. that which I had already done." I cannot find where he before has stated that which he now says he repeats. This shows the propriety of my asking Mr. Murray where is his previous statement?

I have not copied the whole of Mr. Murray's paragraph, because some of it is irrelevant to the subject. But when I take the whole of it into consideration, I conclude Mr. Murray means for your readers to suppose, that his apparatus and mine are the same, and that we are only contending for the priority of invention. A mere view of the two plans will convince any unprejudiced person that they are unlike both in construction and effect.

My apparatus, by drawing and returning the pistons, exhausts the lungs, and immediately refills them with fresh air; and this change is continued as long as the pistons are thus moved, without any further attention of the operator; and it also has the advantage of giving to the lungs a similar motion to that of breathing.

Mr. Murray's apparatus is to give motion to the lungs, but to exclude fresh air from them after the first portion has been injected; without natural respiration returns.

I will request you to refer to Mr. Murray's drawing and description of his apparatus, Phil. Mag. vol. lviii. p. 277, where Mr. Murray's words are: "The stop cock is so constructed, that when the handle is parallel with the pipe, as in the figure, there is a free communication established between the lungs and the cylinder, to the exclusion of external air: when, on the other hand, the cock is turned the quadrant of a circle, *the communication with the lungs is cut off*, and there is a free channel opened between the cylinder and the external atmospheric air."

You will now observe, should the operator through hurry or forgetfulness not return the cock from the quadrant of a circle to be parallel with its pipe, the unfortunate being on whom Mr. Murray's plan of resuscitation has been attempted, will be in a most pitiable situation; because his lungs will be in a partial vacuum, and cannot receive any supply of air from the atmosphere. The operator in addition to this is liable to two very considerable errors, *the consequence of this cock*, which will be evident to all who may investigate the plan, therefore I have no occasion to mention them. Mr. Murray says: "I have given *my reasons* for rejecting the cumbrous and troublesome modification obtruded. These reasons remain inviolate."

Gentlemen, I beg you to refer to his reasons, Phil. Mag. vol. lix. p. 374. You will there find that *some one* (for Mr. Murray does not tell who) immediately rejected *some apparatus of his invention* from its *complete uselessness*:—annexed to this, he states it was also abandoned on two other conditions. I should have thought that a machine that was *rejected from its complete uselessness*, did not require two other conditions for it to be abandoned by such a man as Mr. Murray. But as he had there stated that his useless invention was somewhat similar to mine,—to settle this point, I asked him in my previous communication to you, if his was not *dissimilar to mine in some essential part?* This, Gentlemen, was one of the questions I proposed to him, which remain unanswered. You will therefore perceive, that this paragraph of his refers to an invention of his, which some one has condemned, and by his publishing the same has sanctioned as completely useless. This reference must have been nonsensical on his part; because it could only tend to remind us of an invention of his that cannot be any credit to him.

Mr. Murray states: "The Reply is a mere tissue of questions; fourteen marks of interrogation are interspersed! A very convenient mode of reply, it must needs be confessed. For instance, I am asked how the individual becomes reanimated? This is introduced as a species of climax to a most disingenuous (I shall not term it wilful or malignant) perversion of my language.

Gentlemen, What a tissue of questions mine must have been, *that Mr. Murray should have garbled one, noticed all,* BUT ANSWERED NONE OF THEM!!

Mr. Murray says I asked him how the individual becomes reanimated. Here he has garbled my question, even whilst he was complaining of my perverting his language. To convince you who is guilty of the wilful or malignant perversion of language, I will transcribe from period to period that which *I did state*, which I shall place on one side, whilst that, which *Mr. Murray said I did state*, I will place on the other side.

My question to Mr. Murray, Phil. Mag. vol. lx. p. 64, was, "If it be as Mr. Murray has stated, that the air undergoes no change whatever, how is it the individual becomes reanimated? He says it is absurd to give a continued supply of fresh air to an individual, until the natural respiration returns."	Mr. Murray's quotation of it with quotation marks, Phil. Mag. vol. lx. p. 186: "Mr. Murray has stated that the air undergoes no change whatever."

Gentlemen, Mr. Murray's statement of his own words is also garbled, for which see Phil. Mag. vol. lix. page 374, and vol. lx. page 186; yet he has placed to them the marks of quotation, and had them printed in *italics*. I will give you one more specimen of Mr. Murray's carelessness. In Phil. Mag. vol. lix. at the top of page 374, he has actually stated I signed my name Mr. John Moore, Jun. for he has placed quotation-marks to Mr.

He may say, that in Phil. Mag. vol. lx. p. 172, he has given an excuse for his mis-statements. It is, "(for in truth I very seldom read over what I have written, certainly never take or retain a copy of my communications.)" If a man publishes without reading what he has written, the meaning thereof may not only be that which he did not intend, but also that which he cannot find an argument to support.

If Mr. Murray had read two or three times some of his communications which refer to me, I think they would have been divested of several words which they now retain, and been otherwise much improved.

Gentlemen,
I remain respectfully, &c.

Lawrence-Hill, Bristol, 26th Oct. 1822. JOHN MOORE, Jun.

LXVIII. To

LXXVIII. *Inquiry respecting Floods in Dorsetshire.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—I SHALL be much obliged to any of your readers, who will give me such information as they are able to do, respecting a great flood in Dorsetshire many years ago.

The information, perhaps, may be obtained from the *Sherborne Mercury:* which I believe (but do not know that it did) existed at the time in question. If so, perhaps some one would oblige me by looking at the old newspapers in the British Museum for the intelligence desired.

I also wish to know the dates of any floods in Dorsetshire, in the winters of 1750-1, 1751-2, and 1752-3.

AN INQUIRER.

4th November, 1822.

LXXIX. *Notices respecting New Books.*

A View of the Structure, Functions and Disorders of the Stomach, and Alimentary Organs of the Human Body; with Physiological Observations and Remarks upon the Qualities and Effects of Food and Fermented Liquors. By Thomas Hare, F.L.S. &c. Fellow of the Royal College of Surgeons in London. Longman and Co. 1821. pp. 300. 8vo.

THE physiological history of man forms an especial feature of this work, and renders it more extensively interesting than the title-page would lead us to expect, notwithstanding the whole subject is of universal concern. Our limits allow us only to notice, in a brief and cursory way, certain parts of its original physiology, and which is well illustrated by plates from the author's drawings. The contractile and convulsive actions of the stomach and intestines lead to a curious explanation of the *structure of muscular fibre* and its adaptation to the exercise of its mechanical functions; while illustrations of the *ultimate fibre*, not only by its own character and properties, but by comparison with those of vegetable and mineral matter, seem sufficiently to establish its *tubulated structure*, contrary to such opinions as have been adopted of its density, and of its being infinitely divisible. The progress of the work affords opportunities for various chemical considerations, and particularly the influence of alimentary objects on chronic diseases. The manner in which fermented liquors act on the brain and nervous system, so as to produce those aberrations of perception which accompany drunkenness, is among the most interesting of these: and after his views of scrofula, of atmospheric influence, and local station, Mr. Hare concludes his work with the natural history, physiology and diseases of what he properly terms "those primary instruments of the alimentary process the teeth, his researches on which subjects lead him to believe,

"I. That enamel is constituted by a longitudinal arrangement of prisms; and that each prism is made up of an aggregation of molecules, each distinct figure of which is a rhomb.

"II. That longitudinal cracks of the enamel are directed by the corresponding arrangement of its prisms; and that they may be rendered undulating, as they often appear by the rhomboidal figure of those molecules, which in the aggregate compose the prism.

"III. That the arrangement of the rhomboidal molecules directs the diagonal fracture of the enamel.

"IV. That the aggregation of rhomboidal molecules mechanically facilitates the disintegration of the enamel by compression from the lateral surfaces of adjoining teeth, thereby giving extraneous matters an opportunity of acting at large upon the whole substance of the tooth compressed.

"V. That the aggregation of the molecules is favourable in particular to the insidious agency of acrid and decomposing fluids, which are modified as to their chemical influence by the state of the stomach, by various matters of aliment as soon as they come within the lips, and by various nostrums which are used for cleaning the teeth, and, according to the language of quackery, 'strengthening the gums.'"

It will be seen, from the foregoing glance at a few of the subjects of this volume, that it contains much, not only to engage the attention of the philosophical as well as the medical student, but to interest and instruct the general reader.

A Celestial Atlas, comprising a Systematic Display of the Heavens, in a Series of Thirty Maps. Illustrated by Scientific Descriptions of their Contents: and accompanied by Catalogues of the Stars. With Astronomical Exercises. By Alexander Jamieson, A.M.

We congratulate the astronomical student, and the lovers of one of the most delightful of sciences, on the valuable help they will receive from Mr. Jamieson's Celestial Atlas, which is not only calculated to facilitate the study of astronomy, but to give a new charm to the contemplation of the starry heavens, by pointing out how it can be rendered most agreeable and instructive.

The idea of a Celestial Atlas was first suggested by our immortal countryman Flamstead, whose great Atlas was the basis of M. de la Caille's *Atlas Cœlestis*; for the latter merely reduced the English astronomer's maps, and rectified the stars for the period when he undertook the reduction. What M. de Caille did with Flamstead's Atlas, M. Bode did with the *Atlas Cœlestis*, in his *Vorstellung der Gestirne;* which has been the only Atlas that for some time obtained the estimation of astronomers on the continent of Europe, or indeed in this country. But while Mr. Jamieson has availed himself of the labours of his predecessors, he has not been a servile copyist: on the contrary, his work differs as much from those we have mentioned as it is well possible for books to differ, written in illustration of the same subject.

The Celestial Atlas may be divided into three distinct parts. The first, which is introductory, is allotted to some brief definitions, and the manner of using the maps, so as to render them of as general utility and of as easy application as the celestial globe. By means of these maps, a complete knowledge may be gained of the rising, culminating, and setting of the stars, the situation of the planets, the place of the moon, and all the positions into which the grand machine of the universe is successively put throughout the year.

The second and descriptive portion of the work, treats of the boundaries and contents of the several constellations, and the signs of the zodiac. The student who makes himself master of this part of the work will become as familiarly acquainted with the names and situations of the stars, as with the localities of places on a geographical map.

The third distinct part of the work consists of a series of well-constructed Exercises for acquiring a knowledge of the successive appearances of the constellations and signs, in the evening, at midnight, or in the morning, throughout the year. These Exercises are written in a popular language, without any regard to the learned phraseology of astronomers. Indeed, throughout the whole work, Mr. Jamieson's great object appears to have been to render the subject familiar to any ordinary capacity, disdaining all ostentatious display of learning, and anxious only to be understood.

We have only to add, that the engravings are worthy of the subject: they are in the best style of Neele, and are beautifully coloured.

Part II. *of the Philosophical Transactions for* 1822 has just been published: The following are its Contents:

Experiments and Observations on the Development of magnetical Properties in Steel and Iron by Percussion. By Wm. Scoresby, Jun. Esq.—On the Alloys of Steel. By J. Stodart, Esq. and Mr. M. Faraday.—Some Observations on the buffy Coat of the Blood, &c. By John Davy, M.D.—On the Mechanism of the Spine. By Henry Earle, Esq.—Of the Nerves which associate the Muscles of the Chest, in the Actions of Breathing, Speaking, and Expression. By Charles Bell, Esq. —Experiments and Observations on the Newry Pitch-stone, and its Products; and on the Formation of Pumice. By the Rt. Hon. Geo. Knox.—Observations on the Changes the Egg undergoes during Incubation in the common Fowl, illustrated by microscopical Drawings. By Sir Everard Home, Bart.—Some Observations on Corrosive Sublimate. By John Davy, M.D.—On the State of Water and Aëriform Matter in Cavities found in certain Crystals. By Sir Humphry Davy, Bart. —Some Experiments on the Changes which take place in the fixed Principles of the Egg during Incubation. By William Prout, M.D.—On the Placenta. By Sir Everard Home, Bart. —Of the Geographical Situation of the Three Presidencies, Calcutta, Madras, and Bombay, in the East Indies. By J. Goldingham, Esq.—Of the Difference of Longitudes found by Chronometer, and by correspondent Eclipses of the Satellites of Jupiter; with some supplementary Information relative to Madras, Bombay, and Canton; as also the Latitude and Longitude of Point de Galle and the Friar's Hood. By J. Goldingham, Esq.—Observations on the genus *Planaria.* By J. R. Johnson, M.D.—Some Experiments and Researches on the saline Contents of Sea-water, undertaken with a view to correct and improve its chemical Analysis. By Alexander Mercet, M.D.—On the ultimate Analysis of Vegetable and Animal Substances. By Andrew Ure, M.D.

The Cambridge Philosophical Society has just published a second half-volume of Transactions: it contains the following papers:

Analysis of a native Phosphate of Copper from the Rhine: by F. Lunn, Esq.—Upon the regular Crystallization of Water, and upon the Form of its primary Crystals: by Dr. E. D. Clarke.—On the Application of Hydrogen Gas to produce a moving Power in Machinery; with a Description of an Engine which is moved by the Pressure of the Atmosphere upon a Vacuum caused by Explosions of Hydrogen Gas and Atmospheric Airs: by Rev. W. Cecil.—On a remarkable Peculiarity in the Law of the extraordinary Refraction of differently coloured Rays exhibited by certain Varieties of Apophyllite: by J. F. W. Herschel, Esq.—Notice of the Astronomical Tables of Mohammed Abibeker Al Farsi, two copies of which are preserved in the Public Library of the University of Cambridge: by Rev. Prof. Lee.—On Sounds excited in Hydrogen Gas: by Professor Leslie.—On the Connexion of Galvanism and Magnetism: by Rev. Professor Cumming.—On the Application of Magnetism as a Measure of Electricity: by Rev. Professor Cumming.—A Case of extensive Solution of the Stomach by the Gastric Fluids after Death: by Dr. Haviland.—On the Physical Structure of the Lizard District of Cornwall: by the Rev.

Professor Sedgwick.—On Double Crystals of Fluor Spar: by W. Whewell, Esq.—On an Improvement in the Apparatus for procuring Potassium: by the Rev. W. Mandell.—Notice of a large Human-Calculus in the Library of Trinity College: by Rev. Professor Cumming.—On a Dilatation of the Ureters, supposed to have been caused by a Malformation of their Vesical Extremities: by J. Okes, Esq.—Geological Description of Anglesea: by J. S. Henslow, Esq.—Some Observations on the Weather, accompanied by an extraordinary Depression of the Barometer, during the Month of December, 1821: by Rev. J. Hailstone.—Extract from the Minute Book, &c.

Vol. V. Part I. *of the Transactions of the Horticultural Society* has just been published: The contents are as follows:—

On the Species, Races, and Varieties of the Genus *Brassica* (Cabbage), and the Genera allied to it. By M. De Candolle.—On Horizontal Espalier Training. By Mr. John Mearns.—On Chinese Horticulture, Agriculture, and on Esculent Vegetables used in China. By John Livingstone, Esq.—On the House Management of Peaches and Nectarines. By Mr. P. Flanagan.—On Accidental Intermixture of Character in certain Fruits: On a Collection of Pears from the Garden of the Luxembourg at Paris. By Mr. John Turner.—Additional Account of the New Hybrid Passifloras: Further Account of Chinese Chrysanthemums. By Joseph Sabine, Esq.—On the Destruction of Caterpillars on Fruit Trees. By Mr. John Sweet.—On Tropical Fruits worth Cultivating in England. By Mr. John Lindley.—On the Cultivation of the Pine-apple: On a New Variety of *Ulmus suberosa*, and a successful Method of Grafting Tender Scions of Trees. By Thos. And. Knight, Esq.—On the Fruit of Fig-Trees: On the Effects produced by Ringing upon Fig-Trees, and on their Cultivation. By Sir Charles M. L. Monck, Bart.

Recent Publications.

Geological Essays; comprising a View of the Order of the Strata, Coal-fields, and Minerals of the District of the Avon. By Joseph Sutcliffe. 8vo.

A Treatise on Practical Gauging. By A. Nesbitt and W. Little. 12mo.

Zoological Researches in the Island of Java, &c., with Figures of Native Quadrupeds and Birds. By Thomas Horsfield, M.D. No. IV. 4to.

Star Tables for 1822. By T. Lynn. No. II. Royal 8vo.

The Elements of Astronomy. By John Brinkley, D.D. 8vo.

Astronomische Hulfstafeln fur 1822. 8vo.

An Analytical Dictionary of the English Language. By D. Booth. Part I.

A Treatise on the Morbid Respiration of Domestic Animals. By Edward Causer, Surgeon, late Veterinary Surgeon to His Majesty's 4th Regiment of Dragoons. 8vo.

A Manual of Practical Anatomy for the Use of Students engaged in Dissections. By Edward Stanley, Assistant Surgeon to St. Bartholomew's Hospital. 12mo.

Practical Observations on the Nautical Almanack and Astronomical Ephemeris. By James South, F.R.S. 8vo.

Anatomical and Physiological Researches. By Herbert Mayo. No. I. 8vo.

Outlines of the Geology of England and Wales. By the Rev. W. D. Conybeare, F.R.S. M.G.S. &c. and Wm. Phillips, F.L.S. M.G.S. &c. Part I. 8vo.

First Elements of the Theory of Series and Differences. 4to.

Preparing for Publication.

Chevalier **Dupin**, Member of the Royal Institute of France, and author of *Voyages dans la Grande Bretagne*, is about to publish the second part of his Mathematical Researches, under the title of *Applications de Géométrie et de Méchanique*. This work, which form a 4to volume, with 16 plates, contains the author's theories upon the stability of floating bodies; upon the researches relative to the best construction of roads over soils of every kind, in various circumstances; upon the removal of any heap of materials of a specified figure, and forming therewith another heap of a required figure (*deblai et remblai*);—upon the laws according to which, rays of light, emitted from a single point, are subjected in their various reflections from mirrors of any form; finally, upon the mathematical examination of the new structure of English men of war.

ANALYSIS OF PERIODICAL WORKS ON ZOOLOGY AND BOTANY.

Sowerby's Mineral Conchology. No. 65.

Pl. 372. *Ampullaria ambulacrum*, one of the most common shells from that mine of fossil conchology, Hordwell-cliff. Mr. Sowerby has very well distinguished it from two or three other kindred species. Regarding the generic situation of these shells, we cannot agree with either Cuvier or Lamarck. That they have never been found excepting in marine formations, is a strong presumption against Cuvier's opinion that they belong to the *Ampullariæ*, of which all the recent species are fluviatic; while on the other hand we cannot possibly discover why they should be separated from *Natica* as a distinct genus, according to the supposition of Lamarck. We think, if any of the species from Hordwell-cliff are placed by the side of our common English *Natica glaucina*, not a shade of difference in their formation will be observed. *A. ambulacrum* is defined as "globose, with a canal around an acute spire, umbilicus plain within."

Pl. 373 contains two new *Naticæ*. *N. patula*: "Hemisphæroidal, smooth; spire small, depressed; umbilicus open, containing a spiral ridge;" found in the Suffolk crag. *N. striata*: Subhemisphærical, smooth: spire small, depressed; umbilicus open, base concentrically striated. Mr. Sowerby's observation on the difference between these two shells evinces much judgement. We wish he had not followed the example of Dr. Leach, in substituting the little-known and unmeaning name of *Acteon* for that of *Tornatella*, as applied to the generic appellation of *Voluta tornatilis*. De Montford's system has sunk into that oblivion it so justly merits, almost as soon as it was built, and it may serve as a warning to those young conchologists who fancy their reputation will be advanced by opposing established authorities, and building up systems of their own. *Tornatella Noæ*, Tab. 374, as Mr. S. conjectures, appears a very uncertain species. The next Plate contains 3 shells. *Buccinum junceum* (*Murex junceus* of Brander). *B. sulcatum*: Turrited, acute, transversely furrowed; whorls ventricose; aperture ovate; lip toothed within. *B. Mitrula*: Turrited, acute, costated; aperture elongated, obtuse above; lip sharp-edged with a small rounded sinus in the upper part. The first and last of these shells, *B. junceum* and *Mitrula*, evidently belong to the genus *Pleurotoma* of Lamarck. The descriptions of the remaining plates are postponed.

Sowerby's Genera of Shells. Nos. 7 and 8.

We shall notice the genera contained in these Numbers in the order in which they follow in the letter-press. CHAMA: the species represented on this plate are *Ch. Damæcornis*, *Arcinella*, *Lazarus* and *squamosa*. SOLENIMYA: (a name judiciously adopted from Mr. Bowdich, in preference to Lamarck's Solemya,) consisting of two species, to which Mr. S. has added a third with some doubt and without any description. ISOCARDIA: with figures of *I. Cor*, *Moltkiana* and *Basochiana*. IRIDINA; a fresh-water genus of great rarity, well described, and accompanied by a beautiful plate of *I. elongata*. The author, however, is not aware of a second species being figured by Bruguiere; and a third has recently fallen under our own inspection. LIMNEA: Mr. Sowerby, as we before observed, has united the genera *Limnea* and *Physa* of Lamarck and Draparnaud, together with those of *Aplexa* and *Myxas*, all under this genus. We do not at present concur in this alteration, being more inclined to keep *Limnea* and *Physa* distinct, attaching to the first the sub-genus *Aplexa*, and to the latter that of *Myxas*; thus placing these solitary shells as connecting *species*, instead of connecting *genera*, between *Limnea* and *Physa*. We would, however, recommend the perusal of Mr. Sowerby's observations, which are drawn up with great clearness. Ten very accurate figures are given of as many species, but only two of these are accompanied by descriptions. ANASTOMA: a genus formed of the *Helix ringens* of Linnæus, and a *Helix* we wish it to remain, though it has had various other names, and the small variety has been made into another species; we consider it a variety, because we know that in this, and other *Helices*, the number of teeth is always inclined to vary. GLYCYMERIS: the habits of this shell, when compared with many Solens, will bring the two genera as closely as possible together, and *Solenimya*, as Mr. S. conjectures, should range in the same group. LITHOTRYRA: this is a very extraordinary genus, quite new to us, belonging to the Linnæan Barnacles, and to Lamarck's class of *Cirripedes*. Mr. S. gives a detailed account of the only species he has met with, accompanied by the following generic character:—Testa irregulariter subpyramidalis, lateribus compressis, pedunculo tubuloso tendineoque imposita, octovalvis; valvis contiguis, inæqualibus, lateribus sex, inferioribus minimis; dorsali magnâ, ligulatâ, anticâ minutissimâ. Appendix testacea patellam inversam referens, ad basim pedunculi. Animal intermedium inter sessiles et pedunculatos Cirripedes, saxorum cavos ab ipso terebratos incolens.

From these characters the structure, as we think, will be found at variance with the idea that the animal is able to pierce a deep cavity in limestone rocks; for, as it is elevated on a peduncle, it must in that case detach itself from some other substance, work with its head downwards to permit its coming in contact with the rock, and then again reverse itself, and fix its seat in the bottom of the cavity: all this, we conceive, is highly improbable, if not impossible. We have learnt that all the specimens which have been brought to England, were procured from one rock at Montserrat; and our present belief is, that they belong to a species exclusively inhabiting the deserted cavities *previously* made in rocks by the *Pholadariæ;* precisely in the same manner as the Hermit Crab takes up its abode in empty shells. We do not, however, wish this opinion to be implicitly believed, but merely offer it as the most probable of the two, and until further information may supply the place of conjecture on this very interesting subject. Mr. Sowerby animadverts, with great reason, on the strange and even absurd idea of making *Otion* (a Linnæan Barnacle) a bivalve shell, because it has only two valves! CRENATULA: a genus very well defined, and with two excellent figures. PERNA: Mr. S. is not aware that this genus was one of the last instituted by the great Linnæus, though not published under his name; the honour must not therefore be conferred on Lamarck. Four admirable figures are annexed.

In conclusion.—Anxious as we are for the success and improvement of this work, we must again urge to the authors our strong recommendation to give some descriptive or specific characters to the shells they figure, and also to add references to those authors who have previously figured or described them. This is a duty they owe, not only to the public, but to the labours of those who have gone before them, and whose writings they must of necessity consult, before they can take upon themselves to publish any subjects as hitherto undescribed or misunderstood.

Swainson's Zoological Illustrations. No. 26.

Pl. 125. *Papilio Nireus*, a well-known Linnæan species, but very well figured.—Pl. 126. *Conus vitulinus*, a remarkable variety of this shell not before figured.

Pl. 127 & 128. These plates form a complete illustration of the beautiful cone, vulgarly called the Spanish Admiral. Four distinct varieties are represented with great beauty: the specific character also has been drawn up with precision, and sufficiently proves this shell to be distinct from *C. generalis*, in opposition to the opinion of the Linnæan school: at the same time we perfectly agree with Mr. S., that both Bruguiere, Lamarck and even Dr. Solander, have created in this genus an unnecessary multiplication of species,—an error arising from the variability in the colour and markings of all the cones.

Pl. 129. *Melliphaga torquata*, a species distinguished from *M. lunata* by these characters. M. olivaceo-fulvâ, [illegible]; capite auribusque nigris, torque nuchali lunato, albo, superciliorum cute rubrâ. The bill of this bird is very short; but the nostrils at once point it out as belonging to the Australasian Honeysuckers.

The Botanical Register. No. 93.

This Number is unusually rich in novelties. Pl. 662. has *Gymnoloma maculatum*, a new plant of this little-known genus, from Brazil, thus defined. *G.* aspera, erecta [*um?*], ramis virgatis 4-gonis pictis; foliis oblongo-lanceolatis, subserratis; floribus erectis, terminalibus, 1—3; pedunculis folio plurimum brevioribus; radio 8-floro.

Pl. 663. An unrecorded *Hypoxis*, from the Cape, with the specific name of *stellipilis*. *H.* rhizomate ovato; foliis radicalibus plurimis trifariàm fasciculatis triquetro-subulatis, e pilis brevibus stellatis implexis subtus tomentoso-candicantibus, canaliculo carinaque acutis: umbellâ pauci- (2) florâ. This specific character might have been more simple, but that of *Rhexia viminea* (664) exceeds all bounds; for instead of singling out, as it were, only the two or three distinguishing peculiarities of the species, it is extended to a description of the whole plant. Naturalists in general, who wish to facilitate a knowledge of species—the true end of all systems; or who hope to have their characters adopted, would do well to imitate the unequalled perspicuity, in this respect, of the great Linnæus.

Pl. 665. *Costus angustifolius*, a new and superb plant sent by Dr. Wallich from Nepal, is here recorded as a variety of *speciosus*, from which opinion, however, we totally differ.

Pl. 666. *Begonia argyrostigma*, a singular species from Brazil; the leaves being covered with silver-like spots.

Pl. 667. *Loasa tricolor*, from Peru. We must again advert to the diffuse specific character given to this plant. We shall, however, copy it, as the subject is singular and interesting. *L.* urens, erecta, foliis oppositis, bipinnatifidis, ambitu angulari cordato; calyce petalis æquali; coronæ foliolo singulo extus caudiculis subtrinis linearibus diffusè prostratis ad imam baseos marginem appendiculato: staminum fasciculis subdecandris.

Pl. 668. *Arum Dracontium* of Pursh, but not of Thunberg: native of North America.

Curtis's Botanical Magazine. No. 430.

Pl. 2356. *Crassula versicolor*, from the Cape; already figured in the Register, Pl. 320, *Andromeda axillaris*, a variety of the plant described by Pursh. *Broussonetia papyrifera*, the Paper Mulberry Tree: a vegetable of little beauty, but most important to the inhabitants both of Japan and Otaheite; the former of which fabricate paper, and the latter a beautiful white cloth, from the inner bark of this tree.

Spigelia anthelmia: considered in hot climates an excellent specific for worms. We add, on our own authority, that it naturally grows in moist shady thickets, and the borders of woods and plantations.

Hovenia dulcis, Willd. *Iris furcata*, from Caucasus.

Tetragonia expansa, New Zealand Spinach. This plant appears to be a valuable addition to our esculent vegetables; and its cultivation has recently been strongly recommended by Mr. Anderson, in the 4th vol. of *Horticultural Transactions.*

Plate 2363 terminates this Number, with *Statice Ægyptiaca*, a species recently introduced by P. B. Webb, Esq.

Geraniaceæ. No. 34.

Only one real species is in this Number, *Horia nutans*, which is thus defined:—*H.* acaulis, scapo diviso, umbellis capitatis congestis depressis, floribus nutantibus, foliis bipinnatis hirsutis: foliolis pinnatifido-laciniatis multifidis subdentatis; petalis superioribus refractis, inferioribus concavis conniventibus. This plant is the *Pelargonium rapaceum* of Bot. Mag. pl. 1877.

Loddiges' Botanical Cabinet. Part 66.

Many of the beautiful and little-known *Orchideæ* of the South of Europe, introduced by the exertions of Mr. Swainson, are now becoming known to the gardens of Britain, and appear in our monthly publications; among these we notice *Serapias Lingua*, a very extraordinary plant, figured in the work now before us, several roots of which Mr. S. sent to the Liverpool garden from Sicily. *Ophrys lutea* is another species figured in the last Number of Dr. Hooker's Exotic Flora, and others are contained both in the Botanical Register and Curtis's Magazine. We have seen, besides these, drawings by Mr. Swainson of many other very singular and unknown species of this interesting family, which we understand he has an intention of publishing at no distant period.

Monograph of the British Grasses.

Of the two first Numbers of this neat and useful work, each contains twelve plates; the third has however but six, but the price is three instead of six shillings. The alteration is satisfactorily explained by a note, which states that it has originated in a wish of proving, by experiment, the various facts communicated by correspondents. Our limits will not permit us to enumerate the contents of each number; nor, indeed, would it be attended with much advantage; but should any new species appear in the course of the work, their characters will be fully detailed.

LXXX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

Nov. 7.—An Appendix was read to a former paper, by John Pond, Esq. Astronomer Royal, on the changes which appear to have taken place in the declination of some of the fixed stars.

14. On the parallax of α Lyræ, by John Pond, Esq. Astronomer Royal.

21. The Croonian Lecture; Microscopical observations on the suspension of the muscular motions of the *Vibrio Tritici*. By Francis Bauer, Esq. F.R.S., F.L.S.—A part only of this paper was read.

LINNÆAN SOCIETY.

The first and second meetings for the season were held November 5th and 19th: both evenings have been occupied in reading part of Remarks on the Identity of certain General Laws which have been lately observed to regulate the Natural Distribution of Insects and Fungi; by W. S. MacLeay, Esq. The admirable and profound work, *Horæ Entomologicæ*, not long since published by this gentleman, contains views of the natural series of animated beings, which, though founded on a close attention to their entire structure, may have appeared extraordinary as well as novel: and it is to a very remarkable confirmation of these views that the present interesting paper relates;—as it appears, from the part which has been read, that M. Fries, in his *Systema Mycologicum* published last year, observes laws of the same kind to obtain in the natural arrangement of Fungi, which Mr. MacLeay had pointed out as existing in the animal kingdom, and as probably extending to all organized beings.—The skins of several rare birds from the East Indies were presented from Major General Hardwicke.

ASTRONOMICAL SOCIETY OF LONDON.

Nov. 8.—This Society resumed its meetings this evening. The papers read were: 1°. A letter from the Rev. L. Evans relative to the mode of determining the intervals of the wires of a transit instrument. 2°. A paper from M. Littrow on the method of correcting the principal errors of the transit instrument. M. Littrow's method differs very little, in its results, from the mode proposed by Delambre, Bessel and others: but his investigation of the principles, on which the method is founded, is at once clear and convincing, and shows the perspicuity and accuracy of this distinguished astronomer. A number of very valuable works (principally foreign) were presented to the Society.

LXXXI. *Intelligence and Miscellaneous Articles.*

ASTRONOMICAL INFORMATION.

1. MR. SCHUMACHER of Copenhagen, to whom the public is indebted for an annual volume of astronomical tables for the use of an observatory, has commenced another work which will also be highly useful to the practical astronomer. It is entitled, *Sammlung von Hülfstafeln*, or " A collection of auxiliary tables." It is printed in octavo, uniformly with his *Astronomische Hülfstafeln*; and the first Number (to which is annexed an *English* preface,) has just made its appearance. This number contains Tables for converting sidereal time into

mean time; Tables of refraction according to Bessel, Brinkley, Carlini, Laplace, Gauss, and Young; Tables for the reduction to the meridian; Tables of equal altitude, computed by M. Gerling from the formula of M. Gauss, for *every minute* of the half interval; Tables for the reduction of barometrical observations; and Tables for finding the sidereal time at mean noon. M. Schumacher does not give any intimation of the extent of these tables: but, in his hands, the selection cannot fail of being judicious and useful.

2. The astronomers on the continent appear to have been actively employed during the present year in observing the three comets, which were first discovered by M. Pons, at Marlia, and subsequently by other astronomers. From the north to the south of Europe, the journals are filled with observations and calculations relative to these singular visitors of our system. Every principal observatory, from Prague to Milan, appears to have directed its attention to these objects: and the favourable state of the weather has enabled the observers to pursue their researches with unexampled success. Amongst those who have devoted their valuable time, either in making observations, or in calculating the orbits of these comets, we observe the names of Pons (*le préposé du ciel*, as he is called by Zach) Carlini, Santini, Ursin, Schumacher, Gambart, Biela, Hallaschka, Caturegli, Frisiani, Olbers, Harding and Enke. It does not appear that either of these comets has been seen, or even looked after, in *this* country. They have, in fact, scarcely been heard of, except through the vague notices in the newspapers.

3. The indefatigable *Bessel* has commenced an important work, which every lover of astronomy must wish to see followed up with success. It is a general survey of the heavens, in zones: and the first part of the work is already in the press. We propose to give, in our next Number, his preliminary remarks on this subject; which will preclude the necessity of our entering further on it at present.

4. There is reason to believe that the observations of the stars given in Lalande's *Histoire Céleste* are about to be reduced: a laborious undertaking, which has long been a desideratum with astronomers.

5. In the Nautical Almanac for 1825, the mean places of the principal stars for January 0. 1825, inserted at the bottom of the table of apparent places, are most of them wrong. Thus the AR of *Polaris* is 0^{h}. 58′. 17″,40 instead of 16″,06: the North Polar Distance of *Capella* is 44°. 11′. 26″,2 instead of 28″,3: the North Polar Distance of *Regulus* is 77°. 10′. 51″,9 instead of 50″,1: and thus of several others. We notice that the longitudes and latitudes of the nine principal fixed stars,

from which the lunar distances are computed, have been recalculated for the beginning of the year 1820. At the bottom of that little table we observe a short *note*, which *may* be correct; but which the practical astronomer will be cautious in adopting *generally*, unless the grounds on which it is founded are distinctly stated.

6. M. Harding has just completed the *last* Number of his *Himmelscharten*: it only remains now to form a catalogue of all the stars, which are inserted therein; without which help, the work will lose much of its utility.

7. During the present year, the German astronomers have been engaged in determining the difference of the longitudes of their observatories, by observations of the transit of the moon over the meridian, and certain fixed stars which differ very little from her in right ascension, and are nearly in the same apparent parallel of declination. A catalogue of some hundreds of such stars, as are thus favourably situated, has been from time to time published in the foreign journals. This method is exceedingly simple; and attended with very little trouble or inconvenience: and, by thus knowing before-hand the stars which have thus been agreed on, the practical astronomer is almost sure to find some corresponding observations, from which he may deduce the required results. As the transit of the moon's limb generally forms one of the objects in an active observatory, the adoption of this method cannot be attended with much additional trouble: since it is merely required to devote a few minutes before and after the passage of the moon, to the observations of the proposed stars: and this may frequently be done without moving the telescope. We intended to have given the catalogue for the ensuing month, for the use of observers in *this* country: but such a measure would answer no good purpose unless some public, or other well known observatory, would undertake to make *corresponding* observations.

8. Mr. Dollond is about to construct a sextant on the principle proposed by M. Amici, as mentioned in our last Number: and there is no doubt that, in his hands, it will receive all the improvement of which it is capable.

9. M. Rumker has commenced his astronomical observations at New South Wales. One of his first objects was to determine the obliquity of the ecliptic, from observations of the sun near our winter solstice, with one of Reichenbach's circles. The quantity which he has deduced is 23°. 27′. 44″,5 for the mean obliquity on January 1, 1822; which differs only 0″,3 from the value deduced from Bessel's observations, in the north of Europe.

10. M. Enke's computations of the comet, whose elements

we gave in our last number, show the path of it to be elliptical; and that it performs its revolution in 194 years.

11. The *Connaissance des Tems* for 1825, has just made its appearance in this country: as well as the *Coimbra* Ephemeris for 1823 and 1824. Bode's *Astronömische Jahrbuch* for 182[illegible] has not yet arrived. The *Milan* ephemeris is always slow in its progress to *this* country: the volume even for 1822 is not *yet* to be procured.

MORDANT FOR CRYSTALLIZED TIN.

A celebrated chemist, now retired to cultivate his land (says the *Giorn. di Fisica*, Dec. II. P. I. p. 217) recommends the following mixture as a mordant for *moire metallique* on tinned iront,—the crystallized tin of artificers.

Sulphuric acid diluted with six parts of water, 3 ounces. Nitric acid, 1 to 2 drachms. Diluted solution of chlorine, 4 ounces. Oxalic acid, 1 to 2 scruples. The juice of an orange.

The brilliant ground may be darkened at pleasure. An addition of ammonia to the liquor makes it darker, and more so a little sulphate or acetate of copper. After the action of the mordant it may again be altered: by carbonate of potash the brilliancy is softened, and raised by caustic potash (either must be well diluted with water).

If a tinned iron of a fine grain is wanted, the mordant must at first be applied, and then the tinned iron be heated in a furnace till the tin begins to melt; it is then taken out and sprinkled with fine drops of water.

ERUPTION OF MOUNT VESUVIUS.

Naples, October 21.

Yesterday, at sunrise, Vesuvius was still tranquil, though for two days the water of the surrounding wells had entirely disappeared: but a few seconds after twelve o'clock, smoke, mixed with lava as usual, began to appear. About two o'clock a dreadful internal noise was heard throughout the whole neighbourhood, and the noise continued to increase until midnight. In fine, about half-past three o'clock a terrible explosion took place from the upper cone, preceded by repeated shocks and internal howlings from the mountain.

The shocks increased gradually to sunrise, and about two hours after sunrise a torrent of lava about a mile broad was perceived, and extending as far as a mile and a half between the Casa de la Favorite and Resina. The terror of the peasants, and of the people who at this season occupy their country-houses, was so great, that the road from Portici to Naples was filled with carriages conveying families and valuables from the scene of danger. According to the latest accounts, the mountain was undergoing great convulsions, and though the weather was serene, a thick cloud of ashes and stones darkened all the left side of the crater, and exhibited a spectacle at once picturesque and awful.

October 22.—Mount Vesuvius showed signs, yesterday evening, of an approaching eruption, and to-day it has broken out. The mountain is all on fire; the continued oscillations which are felt, indicate that part of the crater is fallen in. The branches of lava are numerous; but we cannot distinguish their rapid course, on account of the thick smoke, but their direction seems to follow the ancient track. Several parties of foreigners have determined to go this night to the spot, to observe more closely the effect and progress of the lava. The explosions are frequent; and if the land breeze should blow again to-night, we shall have a shower of ashes similar to that which we had in 1808.

October 23.—The eruption of Vesuvius is terrible. The torrent of lava which flows towards Resina has already covered 100 acres of ground. The showers of ashes darken the sky, and fall even in the streets of the capital. The stones which have fallen at Boscotre Casa have accumulated to the height of five palms.—*Gazetta di Napoli.*

Naples, *(Further Particulars,)* Oct. 20, 1822.

Prayers were this day put up in the church of St. Januarius, for the preservation of this city from the danger with which it was threatened, by one of the most dreadful eruptions of Vesuvius that has occurred within the memory of man. The thanksgiving will continue for three days: the assemblage was immense. The columns of fire, stones, and ashes, which the volcano has vomited forth for many days past, have been of less magnitude to-day, and there is every prospect of a speedy termination of this terrible phenomenon. We have collected the following particulars respecting the progress of the lava:—At first it flowed in a stream of the breadth of half a mile, in a direction west from the mountain; and after destroying a great extent of ground, it stopped at a place called Monte. A second body of lava proceeded from the same crater, and at the same time as the one already mentioned, and covered the old lavas on the side of Boscotre Casa, without doing any mischief. A third stream issued from a new crater which the eruption had opened, and stopped near the first. And, finally, a fourth stream burst from the old crater called Vulcan's Mouth, and took the direction of La Torre. It has been remarked of this eruption, that the matter thrown from the volcano, taken in a mass, far exceeds the lava in quantity. So numerous were the stones cast forth from the volcano, that they filled the Consular road from Resina to the tower of Annunziata, and blocked up the passage. The police, the members of Government, and the Austrian soldiers, rivalled each other in endeavouring to remove this obstruction. The eruption of cinders and smoke at this moment presents the appearance of a very thick and elevated black cone, which the wind blows towards Somma, Ottaiano, and Nola.

October 26.—We expect that the eruption will soon entirely cease. The columns of cinders and smoke are decreasing, and the detonations are less frequent and loud than heretofore. Most of the people who had fled are returning to their homes. It rained copiously last night; which has had the effect of purifying the atmosphere, which before was filled with clouds of black ashes. The rain, too, has washed the plants, which have assumed their natural colour and appearance, which under our climate is, even at the end of autumn, so striking and agreeable. The summit of Vesuvius is visible, and it appears the dreadful eruption which has taken place has torn away a part of the crest of the volcano.

October 28.—The eruption is completely at an end, but violent explosions of cinders still continue. The inhabitants of the country have returned to their homes. Portici and La Torre del Græco have suffered no other injury than what arises from their being in a great part covered with ashes and stones. A portion of the territory of Resina is covered with lava, but only where lava had formerly lain. The tower of the Annunziata has sustained injuries which it will not be easy either to estimate or repair. At Ottaiano the fire has consumed 50 acres of wood. These are all the details which have hitherto reached us.

The same date (four in the evening).—The report which we have just received from Ottaiano informs us of new disasters in that quarter, and of others with which it is still menaced. It is not the fire which is now feared, but terrible overflowings.

SINKING OF THE EARTH IN AMERICA.

Such a phænomenon has taken place, and is still progressing in the country of Jefferson, near the Warren line, on a hill near the Ogechea River, as is not common in this part of the world. About six or eight weeks ago, the earth on a steep hill-side was discovered to be sinking and dividing asunder to the extent of about one acre. A gentleman in the neighbourhood of this scene, told me that he went round it and on it about three weeks ago, and very distinctly heard the cracking and snapping of the roots. A man of the same neighbourhood, who was my pilot to this eventful place on the 25th of June 1822, told me that it was progressing fast. When I was favoured with a view of it, I think it had extended over about two acres. On the most elevated part of the hill, the earth has sunk about 12 feet perpendicular, while on the lower side it has risen six or eight feet above the surface. Over about one acre the timber has been prostrated on the earth, forming a ruinous appearance from its having been thrown in every direction. On the other part some of the trees are fallen; whilst the remnant are tilted in different directions, with a number of cracks of different sizes, and running various courses. There is a large crack extending itself along the side of the hill, indicating thereby the further progress of this strange eruption. Previous to this event there was a good spring of water flowing from the troubled part of the earth; the water still issues from the ruin, resembling in colour the earth which is discoverable in those cracks.—*Georgia Paper.*

SALT STORM.

The dreadful gale that blew at Newhaven, United States, from S.E. on the 3d of September 1821, gradually increased from noon until dark, when it raged with tremendous violence, until near midnight. It terminated very abruptly, and passed in a very short time from a hurricane to a serene and star-light night. Near midnight, a loud report was heard by many, and it was observed that the wind ceased immediately after. Next morning, the windows were found covered with salt; the trees exhibited a blasted foliage; in a few hours, the leaves began to shrink and dry on the windward side, and after some days the dry leaves fell, as they ordinarily do in the latter end of November. In October the leaves re-appeared on the windward side of the trees, new blossoms were put forth, and the water-melon and the cucumber produced new fruits. In some instances, the mature fruit was found on the same tree with the blossoms. On the morning after the tempest, the leaves were perceptibly saline to the taste at Hebron, 30 miles from the sea: and it is stated that the same effect was observed at Northampton, more than 60 miles inland.—*American Journal of Science.*

MANUSCRIPT HOMER.

A letter has been received by Mr. W. Bankes, from Mr. Salt, dated at Cairo, in August last, with the following curious information:— A roll of papyrus, measuring about eleven inches in length and five in circumference, has been discovered in the Island of Elephantina, and purchased for Mr. Bankes. It is found to contain a portion of the latter part of the *Iliad,* very fairly written in uncial letters, such as were in use during the time of the Ptolemies, and under the earlier Roman Emperors. The lines are numbered, and there are Scholia in the margin. A copy is to be made from this valuable MS. at Cairo, that it may serve as a duplicate, in case of any accide befalling the original in its voyage to England. The pers who procured this treasure for Mr. Bankes, is a young man who has been employed for some years to make researches into the antiquities and geography of the East in such parts as were left unascertained by Mr. Bankes himself.

MACHINES PUT IN MOTION BY EXPLODING GASES.

According to the Edinburgh Journal, 1821. April. p. 427. Mr. Cecil, a clergyman, exhibited in the Philosophical Society at Cambridge, on the 15th of November 1820, an engine which was put in motion by repeated explosions of gas. It is known that Dr. Romershausca long ago invented engines for lifting, &c. which may be put in rapid motion by firing small cartouches filled with gunpowder.

Dr. Lamb, F. L. S. of Newbury, the fortunate possessor of the only *Tantalus Falcinellus* (Bay Ibis) ever killed in England, has lately had brought to him the *Lestris crepidatus* (the Black-toed Gull) killed at Shaw, Berks. It is a rare bird in the south, and is thought by some ornithologists to be only a variety of the *L. parasitticus.*

ALKOHOMETRICAL APPLICATION OF THE THERMOMETER.

Mr. F. Groening, of Copenhagen, has discovered that the thermometer may be successfully used in distillation, as an alkohometer. He observed, while comparing the temperature of the interior of the rectifier with that of the water about it, in a distilling apparatus invented by himself, that the thermometer always rose to a certain point,—for example, 65° Reaumur, or 179° Fahrenheit,—before the first drop of the distilled liquor appeared; and likewise, that it remained at that point till about half the fluid in the retort was evaporated; but then, by degrees, at first slowly, afterwards more rapidly, rose to 80° Reaumur, or 212° Fahr.

By trials with the alkohometer, he found that as long as the thermometer remained at a certain point, the liquor which came over was of an uniform strength; but when it rose, the liquor grew weaker and weaker, till at last mere water came over, namely, when the instrument had attained the height of 80° Reaumur.

The results of M. Groening's experiments, which were performed many times, and which of course depend on the different temperatures of the vapours of alcohol and water, were as follow:

1. A person may, by the state of the thermometer, immediately ascertain the strength of the liquor in the vessel.

2. There is no necessity of using the alkohometer in distillation, as the thermometer indicates the strength of the liquor with equal accuracy.

3. Without drawing off any spirit, what quantity there is of any particular strength may be immediately known.

4. Every possible fraud, during the operation, may be prevented, as the apparatus can either be locked up or brought into an adjoining apartment, for the person who attends the work does not require the thermometer to direct him.

EARTHQUAKE.

Latakia, (in Syria,) Aug. 28.

The city of Latakia and its environs have suffered severely by a dreadful earthquake in the night of the 13th of this month. A shock had been felt on the 12th, and it was imagined that all was over, when on the 13th, about 20 minutes past nine in the evening, a slight trembling was the harbinger of most violent shocks that immediately followed. They began from north to south, and then took a direction from east to west. The shock continued for forty seconds.

EARTHQUAKE AT ALEPPO.

A letter from Constantinople, dated Sept. 3, gives the following account of a dreadful earthquake at Aleppo:—

"Aleppo, one of the most beautiful cities of the Ottoman Empire, has been visited by an earthquake, resembling those which laid waste Lisbon and Calabria in the last century. The first and most severe shock occurred on the 13th of August, about ten in the evening, and instantly buried thousands of the inhabitants under the ruins of their elegant mansions of stone, some of which deserve the name of palaces. Several other shocks succeeded, and even on the 16th shocks were still experienced, some of which were severe. Two-thirds of the houses of this populous city are in ruins, and along with them an immense quantity of valuable goods of all kinds from Persia and India have been destroyed.

"According to the first accounts of this event, which through alarm may have been exaggerated, the number of the sufferers amounts to from 25 to 30,000. Among them is one of the best men in the city, the Imperial consul-general, the Chevalier Esdras Von Piccotto. Having escaped the danger of being buried under the ruins of his own house, he hastened with some of his family towards the gate of the city; but as he was passing a Khan, a new shock occurred, and buried him and those with him. Tartars who have arrived from Damascus report that they saw the whole population of Aleppo encamped in the environs. They state that several other towns in the Pachalics of Aleppo and Tripoli, particularly Antioch and Laodicea, have been destroyed by this earthquake. The captain of a French ship also has reported that two rocks at the time of the earthquake had arisen from the sea in the neighbourhood of Cyprus, which is almost under the same latitude as Aleppo.

"As soon as the Arabs and the Bedouins of the Syrian Desert obtained information of the calamity which had befallen Aleppo, they hastened in hordes to exercise their trade of plunder in that immense grave. Behrem Pacha, however, drove them back, and also executed several Janissaries who had committed depredations among the dead bodies and ruins.

"The great number of unburied bodies in this extremely hot period of the year, has produced pestilential effluvia, and obliged the unfortunate inhabitants to seek for refuge in some remote district."

SIBERIA.—CAPTAIN COCHRANE.

The celebrated Englishman, Captain Cochrane, who is famous both in and out of Europe, for his long excursions on foot, and has been for two years engaged in such a tour in Siberia, to discover whether in the high Northern latitudes there is any connexion between the continents of Asia and America, has married in Kamtschatka, a native of that country, and is now on his return. He has not found any junction of the two continents.

AROMA OF RUM AND MEAT.

According to Proust (*Ann. de Chim.* xviii. 176) the peculiar substance of genuine rum is not originally derived from the treatment of the saccharine matter in the syrup, as some persons think; but is a natural aroma of a peculiar sort which exists in the fresh juice of the cane; in the same way as the savoury principle of meat is not generated by its treatment at the fire, but may be extracted by alcohol even from the raw meat, and in fact (as from cheese) in the state of a peculiar acid, on the nature of which Mr. Proust intends to communicate more particular researches.

OBITUARY.—PROFESSOR TRALLES.

We have the painful and unexpected task of recording the death of this much esteemed Professor of Mathematics in the University and Secretary to the Mathematical Class of the Academy of Berlin, after a sudden and short illness while on a visit to this country, on the 19th inst. (Nov.)

Professor Tralles was, as we are informed, a native of Switzerland. He was formerly Professor of Mathematics at Berne in that country, where he became acquainted with Mr. Hassler, late Astronomer under the treaty of Ghent on the part of the United States of America, with whom he undertook an accurate trigonometrical survey of Switzerland, first at their own, and afterwards at the public expense. The French Revolution prevented the execution of the whole of their plans, but the French have, however, partly continued their surveys. When

France invited other nations to send Commissioners to assist the Committee of Weights and Measures, which were designed for universal adoption, the Swiss Republic sent Mr. Tralles, as the Dutch sent Mr. Van Swinden, (the only two foreigners who assisted): and as a compliment to them, these two were requested to draw up reports of separate parts of the committee's labours. Mr. Tralles afterwards became a Member of the Academy of Berlin, in which Academy the vacancies are filled by the existing Members, the Government approving or rejecting the choice. When an University was established in Berlin in 1813, Mr. Tralles became Professor of Mathematics and Astronomy in that University, and delivered lectures to the students. In this situation as Academician and Professor he continued till his death. He married a Swiss lady (the sister we have understood of Sir Francis d'Ivernois), who is now living and by whom he has left some children. His late mission to this country was in order to buy instruments for the Prussian Government. There are several of his papers in the Memoirs of the Berlin Academy; principally on mathematical and geodetical subjects. Geodesy was always his favourite pursuit. He was about 60 years of age when he died.—He attended the first meeting, for this season, of the Royal and Astronomical Societies, and his death has occasioned the greatest regret here among those who had become acquainted with him. He was buried on Saturday the 23d of November, in the church of St. Andrew, Holborn, in this city, and his funeral was attended by the Prussian Ambassador, Consul and Vice-Consul, and by several English gentlemen who respected his talents.

COUNT BERTHOLLET.

Paris, Nov. 8.—Count Berthollet, one of the principal founders of modern chemistry, died last Wednesday, after a short illness. Since the death of Lagrange and Monge, the sciences have not suffered a more severe loss. No man, perhaps, had more friends, or kept them longer; and none was more deserving of happiness, by the elevation of his character, the nobleness and generosity of his sentiments, the constant mildness of his manners: he has left in society as great a vacancy as in the sciences, and as difficult to be repaired. He had attained the age of 73 years and 6 months. His robust constitution made his friends hope that he would live longer, and there was no indication that he was likely to be carried off so soon. The strength of mind with which he bore and concealed his sufferings, for fear of afflicting a beloved wife, can only be compared with the tranquillity with which he saw the hour of death approach. He died at 7 o'clock in the evening.

LIST OF PATENTS FOR NEW INVENTIONS.

To Thomas Leach, of Blue Boar-court, Friday-street, Cheapside, London, merchant, in consequence of communications made to him by a certain foreigner residing abroad, for his improvement in steam-engines, by the application of steam immediately to a wheel, intead of the usual process.—Dated 25th October 1822.—4 months allowed to enrol specification.

To William Piper, of Cookley Iron-Works, in the parish of Wolverley, Worcestershire, civil engineer, for several new anchors for the use of shipping and other vessels.—1st November.—2 months.

To Alfred Flint, of Uley, Gloucestershire, engineer, for a machine for scouring, pissing, and washing, of woollen cloths. —1st November.—2 months.

To John Oxford, of Little Britain, London, gentleman, for his improved method of preventing premature decay in timber, metallic substances and canvass, by the application whereof, to such several bodies respectively, the same are respectively rendered impervious to the dry-rot, damp-rot, worms, insects, or rust, to which the same are respectively liable; and the same are thereby rendered more durable and less liable to decay.—1st November.—2 months.

To John Dowell Moxon, of Liverpool, Lancashire, ship-owner and merchant, for his improvements in the construction of bridges and works of a similar nature.—9th Nov.—6 mo.

To Francis Deakin, of Birmingham, in the county of Warwick, sword-manufacturer and wire-drawer, for an improvement in the manufacture of holster-cases, cartouch-boxes, and certain other description of cases.—9th November.—2 months.

To John Jekyll, of Roundhill-House, in the parish of Wincanton, Somersetshire, captain in His Majesty's Navy, for certain improvements in steam or vapour baths, to render the same more portable and convenient than those in present use. —9th November.—2 months.

To Richard Roberts, of Manchester, Lancashire, civil engineer, for certain machinery or implements applicable to the process of weaving plain or figured cloths or fabrics, which may be used on and in conjunction with looms now in common use; and also certain improvements in the construction of looms for weaving plain and figured cloths or fabrics, and in the method of working looms either by hand, by steam, or other power.—14th November.—2 months.

METEOROLOGICAL TABLE.

The *London* Observations by Mr. Cary, of the Strand.

The *Boston* Observations by Mr. Samuel Veall.

Days of Month.	Thermometer.				Height of the Barom. Inches.		Weather.	
	London.			Boston.				
1822.	8 A. M.	Noon.	11 P. M.		Lond.	Boston	London.	Boston.
Oct. 27	50	56	45	55	29·69	29·30	Fair	Cloudy, Rain A. M.
28	40	57	48	52	·90	29·60	Fair	Fine
29	54	58	52	56	30·00	29·60	Cloudy	Cloudy
○ 30	54	60	54	58·5	29·99	29·65	Cloudy	Rain
31	55	62	56	59	·76	29·35	Fair	Fine
Nov. 1	56	62	57	57	·93	29·50	Fair	Cloudy, Rain P. M.
2	58	60	57	62	·75	29·25	Cloudy	Cloudy
3	54	58	44	55·5	30·10	29·80	Fair	Fine
4	42	55	46	53	·31	29·85	Fair	Fine
5	54	57	52	57	·29	29·80	Fair	Fine
6	50	56	48	57	·23	29·74	Cloudy	Cloudy
7	45	58	46	53	29·95	29·55	Fair	Fine
8	46	50	40	47·5	·99	29·80	Cloudy	Fine
9	38	50	49	42·5	·98	29·70	Foggy	Fine
10	49	53	40	52	·87	29·70	Cloudy	Cloudy
11	42	53	40	50	30·30	29·95	Fair	Fine
12	50	55	48	53·5	·08	29·65	Cloudy	Cloudy
☾ 13	46	50	44	46·5	29·78	29·50	Fair	Fine, Rain P. M.
14	43	50	48	47·5	·55	29·22	Fair	Fine
15	47	50	40	43	·28	28·88	Cloudy	Stormy, Rain A. M.
16	42	40	39	41	·25	29·07	Rain	Rain
17	41	49	48	47	·66	29·40	Fair	Cloudy
18	49	54	50	47·5	·74	29·35	Cloudy	Fine, Rain A. M.
19	50	54	50	53	·84	29·87	Cloudy	Fine
20	52	55	50	51	·65	29·55	Rain	Rain
21	50	53	44	48·5	·75	29·35	Fair	Fine
22	44	50	50	48	·75	29·43	Rain	Rain
23	48	54	46	51	·65	29·16	Fair	Fine
24	45	51	51	50	·90	29·60	Cloudy	Fine
25	46	52	50	51	·55	29·27	Showery	Cloudy
26	46	52	52	49	·59	29·25	Showery	Cloudy

The quantity of Rain in London since the 26th Oct. amounts to 3 In. 5 tenths.

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LXXXII. *On the Origin of the Blast Furnace.* By DAVID MUSHET, *Esq.**

It is a matter of considerable regret, to those interested in the rise and progress of our national metallurgy, that the comparative recent invention of the blast furnace should already be involved in so much doubt and obscurity; and that we should be unable to assign to it "a local habitation and a name."

In the writings of the Saxon Agricola, published in the year 1556, there is no mention made of the blast furnace as known to us in after ages, as the means of making and procuring that strong but fusible state of iron now so universally used in every mechanical art and contrivance. Agricola describes the blast bloomery and the usual processes for obtaining malleable iron, but no where does he describe a process by which cast iron was obtained and applied to foundry purposes. He gives the detail, however, of a curious process for making steel, which renders it probable that the more fusible species of cast iron was not unknown in that day, though not used for the purpose of castings.

Pieces of the most fusible iron were introduced into a large crucible, in the hearth of the iron-furnace, along with a mixture of flux and charcoal; the action of the bellows was directed so as to produce fusion; when this was ascertained, four pieces of bar iron, about 30lbs. each, were inserted in the fluid, where they remained for five or six hours, till they had nearly absorbed the melted iron. The masses of malleable iron, being thus penetrated, enlarged in size and became soft and pasty. The furnace-man then made a trial of one of the bars, placing it under a large or forge hammer. The cake while yet hot was plunged into cold water, and the fracture examined, to ascertain whether it had been all converted into steel: if this was not the case, a fresh fusion became necessary; and the imperfect steel was again inserted for a length of time sufficient for its perfect conversion.

Whether this fusible mixture was in the first instance gray cast iron, or iron containing less carbon, it will hardly be contended that it did not become so when fused in contact with flux, in a crucible containing pounded charcoal. Besides, it is not altogether improbable, that some particular qualities of ore, finely reduced and added in sparing quantities compared with the mass of the fuel (at that time the charcoal of wood), were found to yield, even in the low ancient bloomery, what is now so generally known to us under the name of gray cast iron. The cost of its production, as to fuel, time and labour, would confine its use to some rare and valuable purposes: and as we know of none more useful or more generally valuable in metallurgy than the manufacture of steel, it is probable that the making of the more fusible qualities of cast iron was for the exclusive purpose of fabricating steel; and it is also probable, that gray cast iron was discovered by endeavours to form a metal as fusible as the rude and limited nature of the operation would permit: the fusibility of iron being always in proportion to the quantity of carbon with which it is united.

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* Communicated by the Author.

No circumstance with which I am acquainted, conveys so lively a picture of the state of the arts, as far as regards the manufacture of iron,—than that the most enlightened nation in Europe, as to mining and metallurgy, should in the middle of the sixteenth century possess no other process for making steel, than the one described above; a process which, when performed in a manner much more perfect than that described by Agricola, is yet productive of an inferior and uncertain quality of steel; and in point of expense, compared with the present manner of making steel, utterly impracticable but for the purpose of mere experiment.

The perfection of such a process (however incomplete we may now consider it) may probably have led to a discovery of the manifold combinations of iron with carbon in the furnace: for as dark or gray fractured cast iron contained double the quantity of carbon that united itself to white iron, and four times the quantity contained in the crude steel (the natural product of iron ores when smelted in furnaces not more than three or four feet in height, as must have been the case in the blast bloomery)—so in the same proportion would it be prized, and found more valuable for the manufacture of steel.

For a great length of time, a fusion of this species of cast iron might have been considered as a sacred menstruum into which iron was plunged for its purification, to pass into the more noble, useful, and exalted state of steel. But as the arts advanced, and the genius of war revealed to her ambitious sons the direful effects produced by the inflammation of gunpowder in strong metallic tubes, the attention of the founder would naturally be directed to every known state of iron, to prove its capacity for resisting the violence of the necessary explosion: and as œconomy seldom enters into the views of the warrior, quantity, more than the cost of production, would be the first object of consideration when experiment had determined the superior strength of carbonated cast iron, and its capability of being run or cast into form. The malleable iron artillery with its numerous hoops and cases would be abandoned, and the strongest sorts of cast iron sought after. Demand would stimulate the exertions of the iron-maker, who in time might be led to reason that a larger furnace might produce a larger quantity of iron. The old bloomery furnace would be enlarged: and as every addition to the size, by increasing the period of cementation and contact of the ores with the fuel, would not only increase the fusibility and strength of the iron by additional carbonation, but also materially reduce the cost of production,—a permanently enlarged furnace would be the consequence, though still possessing the form and proportions of the bloomery furnace. A decided advantage having been obtained by the enlargement of the furnace, in producing gray or fusible cast iron, it is probable this improvement would be pushed to the extreme; and long before any adequate improvement took place in the blowing machine, the soft and very limited quantity of blast that was found sufficient to penetrate a column of iron-making materials in the blast bloomery, three or four feet high, would be found of difficult ascent through a furnace of three or four times that height. The combustion would in consequence proceed languidly, the increased height and greater pressure of the ores would repress the blast as it entered the twyre, and a period of difficulty and distress be likely to ensue. In such an emergency as this, a suspending medium was probably first thought of, and introduced into the blast furnace; to which, at the time or since, has been given the name of *boshes:* these, by creating immediately above the twyre a lateral suspension of the entire column of smelting materials, removed the pressure from the central parts of the furnace, and allowed the blast to ascend with more freedom and effect. Whatever benefit was derived from the introduction of boshes into the earliest blast furnaces, it would soon be found, that although by their means quality of iron and regularity of process were obtained; yet quantity, which alone can yield profit, still depended upon some other cause. This evil would be partially remedied by the increase of the size or number of the bellows in those times, chiefly urged by the labour of man or the strength of cattle: but this would increase the number of labourers, the value of the operative stock, and create a great competition among all classes.

Independent of this, the effects of the intermitting and uncertain action of such a power on a large furnace, would entail difficulties of the most ruinous nature; and the iron-maker, at last forced to look out for a cheaper and more permanent blowing power, the rude machinery of the hand and foot blasts would be enlarged, strengthened, and transferred to the motion of the water-wheel. The advantages of local situations would be abandoned, and the iron trade pass from the township in the neighbourhood of the mines, to the banks of the adjacent streams.

In examining the sites of the oldest blast furnaces situated on the upper level of the brooks, the smallness of the stream and the uncertainty of the supply sufficiently indicate the limited operations of the early pig-iron maker; the small scale on which the machinery was erected; and the slow step by which improvement in some ages advances. So long as there was water in the brook sufficient to move the bellows with a certain effect, the operation of blowing continued; when this supply ceased, smelting was at an end for the season; and the labourers dispersed, some to the mines, and some to the woods to prepare materials for another blast.

The first furnaces seem seldom to have exceeded the height of fifteen feet, and six feet at the widest diameter; and the whole capacity not more than four hundred cubical feet. In after times, as machinery became enlarged and improved, and the operations of the furnace better understood, the blast furnace seems by common consent to have been removed to lower levels, where the confluence of several streams gave a more powerful and durable supply of water to the machinery. A good stream of water near to wood, in almost every instance determined the situation of the second and improved class of blast furnaces; locality to the mines was in many instances abandoned, and the ores were carried a distance of eight or ten miles to the furnace.

In the neighbourhood of the ancient forest of Sherwood, this fact seems particularly illustrated; the alluvial soil of that district furnishing only a few specimens of blood-stone, could not have supplied any quantity of ore for the smelting operations of the furnace: the supply of ore seems to have come from the basset edges of the argillaceous veins of ironstone which accompany the coal formation in Derbyshire. The same circumstance occurred in Monmouthshire; the ores of the Forest of Dean were in many instances carried to the furnaces, a distance of eight or ten miles.

The charcoal blast furnace of the present age attained the height of thirty feet, the diameter enlarged to eight or nine feet at the boshes,* and the whole capacity equal in some instances to 900 or 1000 cubical feet.

The first successful experiments for making pig-iron from coke were of course performed in the pre-existing charcoal furnaces of this size; but experience soon found out, that the less active affinity of the carbon of coke for iron and oxygen, required that a longer exposure of the iron-making materials in contact with each other was necessary, to produce profitable results: this could only be done œconomically by an increase in the size of the furnace, and a longer cementation of the ores, in consequence of their prolonged descent. Hence arose blast furnaces which include a capacity of two, three, four, five and six thousand cubical feet: and of late years furnaces have been erected equal to 10,000 cubical feet, without the maximum effect having been decidedly obtained.

But to revert to the age and locality of the blast furnace, with a view to determine the time of its introduction. Whether it is a native discovery, (which I am rather inclined to believe,) or was imported from other countries, I have not been able to determine. The art of making castings from iron possesses no great antiquity in this or any other country; it was unknown, or at least not described, by Agricola in the sixteenth century, who seems to have drawn and described every thing then known of metallurgy; and we possess no traces of the art earlier than the reign of Queen Elizabeth.

I have examined the sites of many old charcoal blast furnaces, with a view of determining their age, by the quantity of slags by which they were surrounded. Here, however, another difficulty has been in every case but one interposed. The manufacture of black bottles has, I think, been traced as far back as the fifteenth century. At what time the manufacture was introduced into this country, I am uncertain; but it is not improbable that in early times, as in the last century, the slags or cinders of the charcoal blast furnace have entered into the composition of black bottles, and created a consumption of that sort of waste which otherwise would have remained in the vicinity of the furnaces. The superior quality of the Bristol black bottles has been attributed to the immemorial use of a portion of the slags of the charcoal furnaces from the neighbourhood of Dean Forest. The consequence of this long-standing practice has been, to carry from the furnaces not only the old slags but those currently made. In one instance only have I found from this source data for calculation. Before the civil commotions of the seventeenth century, the kings of

England were possessed of two blast furnaces in the Forest of Dean, where the cord wood of the forest and the king's share of the mines were used for the purpose of iron-making. Soon after the commencement of the struggle between Charles the First and his Parliament, these furnaces ceased working, and at no period since have they been in blast. About fourteen years ago, I first saw the ruins of one of these furnaces situated below York Lodge, and surrounded by a large heap of the slag or scoria that is produced in making pig-iron. As the situation of this furnace was remote from roads, and must at one time have been deemed nearly inaccessible, it had all the appearance at the time of my survey of having remained in the same state for nearly two centuries: there existed no trace of any sort of machinery; which rendered it highly probable that no part of the slags had been ground (the usual practice) and carried off, but that the entire produce of the furnace in slags remained undisturbed.

The quantity I computed at from 8 to 10,000 tons; a quantity which, however great it may appear for the minor operations of an early period, would yet in our times be produced from a coke furnace in less than two years. If it is assumed that this furnace made upon an average annually 200 tons of pig-iron; and further, assuming the result which has been obtained with ores richer than the Roman cinders, and ores used at that time in Dean Forest,—that the quantity of slag run from the furnace was equal to one half the quantity of iron made (in modern times the quantity of cinder from the coke furnace is double the weight of the iron), we shall have one hundred tons of cinders annually, for a period of from 80 to 100 years. If the abandonment of this furnace took place about the year 1640, the commencement of its smeltings must be assigned to a period between the years 1540 and 1560. If 1550 be adopted as the probable mean, it would from this solitary calculation appear, that pig-iron was made from the blast furnace in England before it was known to Agricola, whose work seems to have been first printed in 1556.

There does not appear from this to be sufficient grounds to suppose that the blast furnace was known in Gloucestershire, or in the adjoining counties, earlier than the middle of the sixteenth century. The local history of Tintern Abbey assigns a later period (the early years of the reign of James the First) for the erection of that furnace. The opportunity afforded of examining both the slags and the iron produced in that early period, abundantly proves that the furnace in Dean Forest above mentioned was one of the earliest efforts in the art of making pig-iron. Small masses or shots of iron are found enveloped in the slags, specimens of iron in a malleable state though rarely, more frequently rough nodules of large-grained steel, resembling blistered steel, and others of a more dense fracture, but of a similar quality. The more fusible reguli of white mottled and gray iron are found in great abundance, all of them possessing forms and appearances of fusion more or less perfect, according to the quantities of carbon with which they are united; and it is but justice to the memory of the father of this art to add, that the specimens of gray cast iron are more abundant than those of the other sorts.

This furnace seems to have been erected upon the spoils of former ages of iron-making, and probably the situation was in the first instance determined by the numerous bloomeries that existed in the neighbourhood; the scoria of which has in after ages been worked to so much advantage in the blast furnace; and though, as a blast furnace, possessed of no great antiquity, yet, as the site of the ancient bloomery, entitled to be considered as the remains of an extensive manufactory of iron in ages more remote.

Upon the whole, several circumstances incline me to the opinion, that the blast furnace must have been known in some of the then iron-making districts of England, before it was introduced into Dean Forest. I saw an account some years ago, to which I cannot now refer, that in the reign of Queen Elizabeth, cannon and mortars of various sizes, and in considerable quantities, were made of cast iron, and exported from England to the continent; which could hardly have been the case, had the invention of the blast furnace, with all its imperfections upon its head, taken place about the beginning of that reign. The oldest casting I have met with in Dean Forest is dated 1620.

The great infusibility and difficulty attending the management of calcareous ores, such as those belonging to Dean Forest, is another circumstance that inclines me to think that the art of making pig-iron did not originate in that quarter; and probably did not succeed entirely till the practice of increasing their fusibility by the addition of the bloomery cinder became known and established. These conjectures are confirmed by reference to a paper in my possession, professing to be an account of all the blast furnaces in England previous to the manufacture of pig-iron from pit coal; probably about the year 1720 or 1730: in which, however, the blast furnace of Tintern Abbey is omitted, and possibly others. At that period there were in all England 59 furnaces, making annually 17,350 tons, or little more than five tons of pig-iron a week for each furnace. This paper I shall subjoin in detail: and my motive is, to exhibit the different iron-making counties of that time: and should it appear that there have been since the invention of blast furnaces, iron-making districts in England in which a greater number of furnaces have been established than in Dean Forest—then to that quarter I should be inclined to look for information on the history, rise, and progress of the blast furnace:—

	Furnaces.		Furnaces.		Furnaces.
Brecon ...	2	Gloucester	6	Salop	6
Glamorgan	2	Hereford ...	3	Stafford ...	2
Carmarthen	1	Hampshire	1	Worcester	2
Cheshire...	3	Kent	4	Sussex	10
Denbigh...	2	Monmouth	2	Warwick ...	2
Derby	4	Nottingham	1	York.........	6

It would appear from this account, that the counties of Sussex and Kent alone contained in the early part of the eighteenth century 14 blast furnaces: and as it is probable that the woodlands in the vicinity of the metropolis would sooner disappear than in the more distant counties, it is equally probable that a century before, the number of blast furnaces might have been considerably greater in that district. The only other iron-making district that will at the time now spoken of bear a comparison with Sussex and Kent, is that of Dean Forest, in which I include the furnace of Tintern Abbey in Monmouthshire, not included in the list; Gloucestershire 6, and Herefordshire 3, making in all, ten blast furnaces.

The nature of this inquiry I feel to be highly interesting; and I hope that this paper will excite investigation in those counties where documents may still exist. In this neighbourhood the change of residence and property has been so entire, as to leave no memorial behind.

Were it necessary to excite attention to a subject so interesting in a national point of view, I might state, by way of contrast to the former yearly make of 17,350 tons, that there are now manufactured annually in Britain, nearly half a million of tons of pig-iron; in the various manipulations attending which, at least five millions of tons of pit-coal are consumed.

In the last published Number of Dr. Brewster's Philosophical Journal, there is a curious account, by Dr. MacCulloch, of a transmutation—shall I call it—of cast iron into plumbago. I am at a loss, from the perusal of the paper, to learn whether Dr. M. considers that a total change has taken place, and that a pound or any other given quantity of cast iron may by this species of disorganization be absolutely and positively converted into an equal weight of plumbago,—a characteristic of which, as given by Dr. M., is purity in proportion as iron is absent. To those who with myself know of no manufactured product of cast iron that contains more than 5 per cent. of carbon, such a circumstance as that now detailed must "give us pause," till further information is given upon the subject.

In a paper so interesting in its details, it is a matter of regret that Dr. M. had not ascertained, by analysis or other experiments, the precise nature of the new product; whether it was by its combustibility a true plumbago, or whether it may not have been a peculiar modification of metallic iron, which upon being made thoroughly dry, pounded, and thrown upon flame, would deflagrate like the filings of iron.

The subject is interesting, and deserves, by further investigation, that attention which Dr. MacCulloch is so well qualified to bestow upon it.

I am, gentlemen, yours, &c.

Coleford, Gloucestershire, Nov. 12, 1822. DAVID MUSHET.

LXXXIII. *Description of a Life-Boat, built by Subscription at Ipswich, and stationed at Landguard Fort; from a Design executed by Mr.* RICHARD HALL GOWER, *Author of several Works on Seamanship and Marine Affairs**.

THIS life-boat is of very light structure, being clencher-built, with half-inch oak plank, and timbered with young ash wood, three-fourths of an inch square, bent to the curvature of the boat. The form of the boat is alike at each end, with a long flat floor, and with a flaring out and unusually projecting head and stern, to meet and lift over the sea. She is decked to 4½ feet from each end, which is covered with stout canvass, and she is steered by a long oar over the stern; the commanding power of which, when properly used, will oblige the bow to face the head-sea, or keep the stern to the following sea; an end-on position with the running sea being essentially necessary to the preservation of the boat, particularly on the occasion of passing her off from and returning through a heavy surf to the shore. These are times of absolute danger; and in case of being filled with water by the staving-in of the bottom, or from the sea heaving too heavily on board her, she is preserved from sinking by the floating capacity of fourteen light cases, which are shaped to meet their several places in the boat beneath the rowing thwarts; viz. a tier amidships, and a tier on each side. These cases are covered with light sheet copper, about twelve ounces to the square foot, and so carefully soldered as to exclude the entrance of the water. There are five cases in each side tier, and four equal cases in the middle tier. Each side tier contains 11½ cubic feet, and the midships tier 27, making 50 cubic feet in cases. Besides this kind of floating capacity, there are 11 feet of cork contained beneath the level of the thwarts at the extreme ends of the vessel. The cases upon an average weigh 8 pounds to the cubic foot; and admitting that a cubic foot of salt water weighs 64½ pounds, each cubic foot of the cases will, on the average, sustain 56½ pounds; and if the cork will sustain 50 pounds, the whole floating capacity will bear up 3375 pounds, or 23 men and 40 pounds, allowing each man to weigh 145 pounds.

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* Communicated by Mr. Gower.

The boat is rigged in the simplest manner, with two snug foul-weather sprit-sails, which take up very little room, and are readily stowed away clear of the oars on each side of the boat, being, moreover, a kind of sail the management of which is well understood by the generality of seamen. Within the boat are two delivering copper tubes of three inches diameter. These pass through her bottom, and are secured by flanges on the outside, and rise up within board to the level which the water takes when it is allowed to flow into the vessel with her crew on board. These tubes will deliver water by self-action whenever its level shall be above their tops and above the level of the water without the boat, and are intended to aid the bailers when their best exertions cannot deliver the sea that is thrown on board. At such times it will be proper to pull out the four plugs in the bottom of the boat, so that the superabundant water above the level within board, when the boat is resting upon her floats, may pass off by this means, as well as through the tubes, and thereby may probably take from the necessity of bailing altogether. The same sized plug-holes will equally deliver with the tubes; but the tubes are always open to meet the occasion, while the plugs may be neglected to be withdrawn.

The general exterior of the boat under canvass, with the steering oar to its position on its pivot at the stern, is represented by fig. 1. From the rounding form of her body she may be considered as bearing a resemblance to some Indian canoe, with the attachment of a keel. In this figure are shown, by dashed lines, the heights of the thwarts and platform, and the several spaces for the stowage of her materials. The floating capacity, in cases, is contained throughout the length beneath the thwarts lettered A and A, and the spaces B and B at the extremities contain the cork. C and C are empty spaces beneath the deck for the stowage of such articles as require to be kept dry, as clothing and provisions. D and D are bailing places, having two plug-holes in each of one inch and a half diameter, with a trap-door to cover over them, to render the platform complete when they are not required to be open. The platform between the thwarts A and A is made to lift, so that the space beneath it may be applied to the reception of such quantity of small tow-line, as it may be considered necessary to have on board, in cases of emergency; and the cable and stern-fast are coiled away on the platform, clear of the bailing places, at E and E. F is a crutch to receive the helmsman's thighs just above the knees, and enables him to stand with firmness on his platform to effect the steerage duty. This is better understood by the bird's-eye view of the thing in fig. 2 at B.

Fig. 2 is a whole-breadth plan of the interior of the vessel, with the position of the thwarts and of the cases of floating capacity on each side and amidships, throughout the length, between and beneath the thwarts A and A; the cases being marked with the letter C. DD and DD are strong cant-pieces or breast-beams across the boat, which limit the extent of each deck; and by rising up about two inches above, they turn all the water over the side, which may be thrown upon the deck, and would otherwise come into the body of the boat. Through holes in the ends of these cant-pieces are wrought four rope-rings or grummets, for the purpose of receiving oars, to assist the steerage, or ropes, on any particular occasion, and the squares by these grummets show the places of four timber-heads: two more also are placed by the foremast; these are convenient to fasten the cable to, and the side ones for head- and stern-fasts when alongside a vessel. All the timber-heads are about ten inches above the gunnel, and are secured by stepping their heels into the thwart beneath, and then bolting them to the breast-beams; and they have a fore-and-aft pin of five-eighths iron passed through their heads, to prevent the turn of a rope from flying off. The darkest shading represents the space left in the boat as foot room for the rowers clear of the floating capacity.

About eight inches below the gunnel, on each side of the boat, is fixed a rack or general handle EE and EE, whereby a number of men may at once take hold of the boat with good effect, and remove her from one place to another, or launch her with readiness into the water. This rack takes the sheer of the boat, and about an inch above it is a cork fender (G G fig. 1) which runs with the sheer of the boat. This fender is four inches deep by two and a half wide, so as to extend beyond the rack as a safeguard to it, and to the boat in case of

a blow.

a blow. It will also, in some measure, secure the stability of the boat on the occasion of a lurch or roll; but independently of this circumstance, and of the general form of the boat, the stability is further insured by the aid of an iron keel of 220 pounds weight. The racks are fitted immediately beneath one of the boat's lands, or projecting edges of her planks; and the cork fenders, which are covered with stout canvass, are secured to their places by copper staples, which are drawn tight to the fender by screw-nuts upon their ends within the boat, being placed at the distance of about 14 inches from each other. The position of the rack and fender is better shown at E and E in the transverse section of the boat, fig. 5.

The copper cases are secured from injury by an external casing of half-inch fir, ledged together into pannels, and secured about them without any nailing. The thwarts also are so dovetailed and hooked to the shelf, or rising of the boat, as to effect the tying of her sides together without the application of knees. This manner of fixing the thwarts, and stationing the panneling, is done for the conveniency of getting at the floating capacity with readiness, for examination, without the assistance of a carpenter; and the mode of doing it is rendered clear by the assistance of fig. 3, which is a midship section of the boat to a half-inch scale.

The lighter shading in this figure, lettered A, A, A, represents the three tier of coppered cases, the dark margin on their tops, and about their sides, being a section of the pannelling. The middle tier of cases, with its pannelling, rests in two grooved sleepers or cants, B and B, which are firmly secured fore and aft to the bottom of the boat, by nails driven from the outside and well clenched within. The lower edges also of the side tier of cases, with their pannelling, are secured by similar cants C and C, which curve up fore and aft along each side; and the upper edges of the pannelling are secured from falling out by flat-headed five-eighths bolts, two inches and a half long, which are driven tight through holes in the thwarts, as represented at D and D. The thwarts are dovetailed into the boat's rising or shelf, E and E, and are further secured by copper hooks or clamps being attached beneath the end of the thwarts, which hook outside the rising, as shown by the dark-lined representation of the thing at E and E. As the thwarts are pressed upwards, in a material degree, by the floating capacity, when the boat is filled with water, their ends are kept from rising by chocks of wood between them and the gunnel; and the middle parts of the thwarts are kept down by iron hooks F and F, which are secured by side-bolts into the cants B and B, and hook into eye-bolts beneath the thwarts, and are secured to their places by short bolts driven through the thwarts at the back of each hook, similar to the bolts D and D, which secure the upper edges of the pannelling. A side-view of one of these hooks is given by fig. 4. The platforms G G and G G, which are made in two lengths of plank, so as to be readily lifted up, rest on ledges GG and GG, which are nailed to the pannelling, and are secured from floating, when water is in the boat, by copper sliding-bolts, which shoot into holes in the ledges of the pannelling.

Fig. 5 is a section of the boat, showing the delivering tubes A A, with the casing or well around them for their security; also the mast as fixed in its step between them. On the outside of this section are several horizontal lines, which mark the boat's draught of water in her various states. The lines B and B show the draught with the crew and her materials on board. The lines C and C represent the draught with the same persons on board, but with the plugs out, and the water inside the vessel being on a level with the water without;—the vessel at this time being rested upon, or entirely borne up from sinking deeper by the floating capacity within the boat. The lines D and D show the draught under similar circumstances, but with seven extra hands on board.

Fig. 6 is a ground plan of the mast-step, which is fayed close down across the bottom of the boat, and there secured. The circles represent the holes through it, which receive the tubes and give them a firm and steady security. The small square between them is the mortice-hole which receives the mast, the dark margin around is a section of the well, and the dashed oblong is the extent of the step across the bottom of the boat.

Fig. 7 represents one quarter of the vessel, and comprehends a sufficiency of lines to exemplify her form to a builder. The general dimensions of the boat are 31 feet from rabbet to rabbet at the stem and stern, and six feet beam from outside to outside; the height from the keel-seam amidships being two feet ten inches, and four feet two inches at the bow and stern.

This life-boat was launched on the 4th of April 1821, in the presence of a large concourse of spectators; when the following experiments were confided to the able management of Mr. Benjamin Hamblin, master of the Stevens, whose readiness to oblige every one with a view of the vessel, in her several states, gave pleasure to all.

In the first instance, the boat was rowed down the river, nearly to the Ballast-wharf, and up again to the bridge, at Ipswich, by six able young seamen, in excellent style, which fairly set forth her ability as a row-boat. In this state, with her crew of seven men, and one extra hand on board, her gunnel amidships was twenty inches out of the water, as represented by the line B B, fig. 5.

Secondly, she was exhibited, as a life-boat, supposed to be filled by shipping more water than bailing would overcome; till, with the plugs out, or as if a plank was stove in the bottom, the water within and without board was upon a level. In this water-logged state she was almost as manageable, and rowed with nearly the same facility as when empty;—the gunnel amidships being 14 inches out of the water, as represented by the line C C, fig. 5.

Thirdly, she was exhibited in the same state, but with seven extra men on board, which is more than is likely to be shipwrecked in any vessel from this port: yet still she continued so perfectly manageable, that Mr. Hamblin remarked he should have full confidence, while in this predicament, with 16 men on board in a sea-way. During this experiment the gunnel amidships was 11½ inches out of the water, as represented by the line D D, fig. 5.

Fourthly, in the same state, but with eight men only on board, she was rowed down the river and back with two men constantly bailing into the vessel, without causing any material increase of the water within-board; for the water within-board must of necessity be above the level of the water without, before it will deliver itself by self-action through the plug-holes in the bottom, and down the three-inch pipes; and when at a *certain* height, it delivered itself so fast through these apertures, that the bailers were incapable of raising it higher. Men were now put on board her, merely to see how many were enough to put all the floating capacity within the boat beneath the water. This was effected by increasing the number to twenty-five, and is fairly corroborative of the calculation which makes the floating capacity capable of sustaining 23 men and 40 pounds, particularly when it is considered that a portion of their weight was in effect taken off by the immersion of their legs in water, and that the men employed might not average 145 pounds.

It must be recollected that the second, third, and fourth experiments are representations of extreme circumstances, and such as can only take place on the occasion of a plank being stove in the bottom; or when, from improper steerage, or on getting into broken water, more sea is shipped than the bailers can overcome, at which time it will be proper to take out the plugs in the bottom of the boat. A boat however of her build, if properly steered, with her head to the sea, or with her stern to the following sea, is not at any time likely to ship more water than a single bailer will overcome; and then, with her plugs in, she will continue a dry boat, and alive to her duty, with more men on board than she could conveniently stow.

Remarks.—In the generality of life-boats the great body of the floating capacity is effected by fixing cork round the outside of the vessel; but as these external projections, when doing their duty by immersion, are a material hindrance to the progress of the vessel through the water, the designer of the present plan has placed the floating capacity within-board. A life-boat, to be perfect, should not only have the property of floating herself and all on board, when filled with water; but she also should possess the capability of being made to obtain the position of the unfortunate who are to be saved. The designer has seen several of the most approved life-boats; but all have appeared to him as too large and heavy for the purpose, and materially wanting the essential property of locomotion; and to be possessed of floatation without celerity of motion, is to render the whole abortive.

Cork is usually employed in life-boats as a floating capacity, and is highly valuable from not being liable to accident; but the best of cork, when closely packed, has more specific gravity, and will take up far more room in the vessel, than the coppered cases here employed to effect the purpose. Cork will also contain a considerable body of water hanging about it, for a length of time, in its inclosed state, which must rot the vessel; whereas the coppered cases, from becoming presently dry, produce no such evil effect.

This life-boat is not provided with a rudder; indeed the form will scarcely admit an effective rudder to be attached to her; and for a life-boat, on her proper duty in heavy weather, a rudder is not only useless, but might become dangerous. A rudder can be of no service to any vessel without way through the water; and the small way obtained on pulling against a high head-sea, is not enough to affect the steerage, when a commanding helm is absolutely necessary to keep the boat's head to the sea. A rudder, therefore, can be of no value to a life-boat; but, on the contrary, it may even prove the cause of her destruction;—for instance, on the occasion of passing to the land to beach the boat through a heavy following sea, breaking with violence into a high surf as it reaches the shore. At such time, by the surf passing the boat with impetuosity, the rudder will be forced across the stern, and the people at the same instant being, in all likelihood, thrown from their oars, the boat, of necessity, under the influence of the rudder alone, will be cast broadside to the sea, and swamped. The commanding oar, therefore, at the stern is the only safeguard on the occasion, and indeed every other; for, even when rowing or sailing in the trough of a high sea, with good way through the water, if a high curling-headed sea is observed to approach the beam, with a prospect of heaving on board, the power of the oar will at once throw the vessel's bow to the sea, and disarm its vengeance, which the less quick power of the rudder would have been incapable of effecting.

In most life-boats the oars are fixed to be rowed through rope-rings or grummets, over a single iron thole like the steerage oar of the present boat, whereby, if quitted by its rower, it is secured to its place. This plan of rowing by a single thole, is much practised by the Spaniards, Portuguese, and others. As few English seamen, however, are accustomed to the mode, and as the life-boat in question was not to have a fixed crew practised to the art, but to depend on such chance crew of volunteer seamen as might be on the spot when the boat was required, it was thought preferable to continue the double English thole, which our seamen are accustomed to, and which allows them to feather their oar with readiness, an art of material moment when progress is to be made against a strong wind and head-sea. These tholes are fitted to the life-boat after the manner practised in the southern whale fishery. Their lower ends are prepared with a hole; and each pair being set in their holes in the gunnel, they are united by having the ends of a piece of small line spliced into the holes, with a spare thole on its bight, as shown by fig. 6. This line should be about 18 inches long, or sufficiently long to allow of the tholes being taken out, and hung within-board, when the boat is alongside any vessel, whereby they are prevented from being lost or broken; and if any are broken while rowing, spare tholes are at hand to supply their places.

It was an object of consideration, not to take from the value of this small life-boat by fitting her out with stores of unnecessary weight and magnitude, as the more she was encumbered and loaded, the less would she be equal to the duties of a life-boat. In consequence, whale-line was adjudged to be the kind of rope best suited to all the purposes of the boat, for cable as well as warp-line. An experienced commander in the southern whale-fishery informed the author, that having harpooned a whale on the coast of Guinea, after a while it sunk dead to the bottom in 13 fathoms water; and his boat remained hanging to the prize by the line from the harpoon in the whale, with three other boats attached to the stern of his boat, till the whale should float by its change of specific gravity. During this period a heavy gale came on to blow towards the shore; but the strength of this single line was enough to hold all the four boats to their object, till the gale abated. This surely is a convincing proof that whale-line is sufficiently strong for the cable, and other purposes of the life-boat in question, the dimensions of which are nearly similar to a whale-boat.

The long parallel form of the vessel may be objectionable in the eyes of a builder; but it was given, not only because it appeared to be a form well calculated as a sea-boat; but also from its allowing a sufficiency of space along the sides withinboard for the stowage of the floating capacity. Moreover, the shape admitted oars of the same length of leverage throughout, whereby all the crew are given the means of pulling with the same strength, and the confusion avoided, which must ever arise when a strange crew come into a boat, the oars of which are adapted by their lengths to particular places.

Where the present boat is to be stationed, she may be launched at most times with readiness; but in such places where difficulties are presented by a heavy surf heaving upon the shore, a rope or chain, from an anchor in the offing, should ever be in readiness to haul out the boat; and if the boat were slung beneath the axletree of two high wheels, such as are used for the carriage of timber, with her bow rather elevated, she may be run out beyond the surf, with all her crew on board, and floated off with much ease and safety by casting off the sling at the proper period. Such a carriage would also be valuable to transport the vessel along the shore to a more weatherly position for reaching the wreck at sea. The wheels for a carriage of the kind should be made not dishing but upright, with a long nave, so that the mortice holes for the spokes may be cut, not central, but alternately round towards each end of the nave, as represented by fig. 9, which is a section of such a wheel. This structure would admit of strength with lightness, which are objects of material moment.

When this design was suggested, the object in view was, to form a small life-boat at a moderate cost, of good floating capacity, and capable of carrying six or seven extra men in safety, even when filled with water, and of so light a structure as to be readily transported from one position to another—possessing also the very essential qualities of a dry and good sea-boat, of steering well, having the capability of being propelled out to sea with facility either by oars or by canvass, and of taking the beach in safety on her return. It was moreover presumed, that were two light boats of the kind to be preserved at the same station, they would prove more effective than one of double the cost and magnitude; for as yet it has not been shown that a vessel of magnitude is more equal to the duties of a sea-boat than a smaller. For instance, where is the boat so alive to its purpose in all weathers as the little Esquimaux canoe? With this fact before us, why are we not, in some measure, to follow the example placed before us by these simple-minded Indians, and effect our purpose in smaller boats, which are less costly and more readily managed? It may, too, happen that enough seamen cannot be collected to man a large heavy life-boat, while a sufficiency may be at hand to man a smaller: but admitting that enough should be collected to man the larger,—with how much more spirit would they proceed to effect their object in two smaller boats, as each would be at hand to help and sustain the other? Indeed, on the occasion of all adventurous boat enterprises, the confidence and emulation, even to heroism, that are created by having boats in company, is well known to every seaman. Each boat is pushing to be foremost to cope with the object, whatever it may be.—Whether on board or on shore, human nature is the same; and on the occasion of a dangerous rencounter, all are more alive to meet it when united than alone.

Had the delivering tubes been of larger bore, they would have effected their purpose with greater facility; or this duty would have been better and more cheaply accomplished by cutting out the plank of the bottom amidships, between the two middle thwarts, to the width of the cases; then by boxing this space up to the height of the present tubes, so as to form a square well, which might be prevented from leaking by a lining of lead or copper, a much greater area would have been given for the self-delivery of the water from the boat.

A boat of this description should be under the constant charge of one active seaman, as coxswain, who is capable of stationing her chance crew to their proper duty, and himself expert at the management of her steerage by the oar at the stern. Without this advantage the boat may almost be considered as useless, and such unfortunately is the case with the boat in question; for she is not only without a fixed coxswain, but at a station, although excellent in many respects, where seamen do not reside.

LXXXIV. *On the Measurement of Timber.* By *Mr.* William Gutteridge.

To the Editors of the Philosophical Magazine and Journal.

St. Fin-barr's, Cork, Nov. 13, 1822.

Gentlemen,—In pursuance of the intimation conveyed in my last letter, which you were so good as to present to the public through the medium of your pages, relative to a change of system in our national weights and measures, I now request your insertion of the following, which I submit with a view of removing an anomaly: to do which, so many ineffectual attempts have been made by mathematical authors.

The property to which I alluded, so much depreciated by the misapplication of measures, is TIMBER; consequently the losers are all those whose estates consist in this article.

I am aware of the difficulty of removing long-standing customs, deeply rooted by prejudice: but as it has been long since made manifest by most able authors, that the established practice of timber-measuring is extremely erroneous, and a shield of protection to designing knavery, common justice demands its abolition.

In support of my position, I shall only quote from Mr. Bonnycastle's Mensuration; because in that work, under the head "Timber-measure," there are sufficient quotations in point from the learned and ingenious Dr. Hutton, and I think more evidence unnecessary. Among many other absurdities of the present mode of measuring, those authors have shown, that if the girt at the greater base of a conical log of timber exceed three times the lesser girt, a part of such log may be taken off, without diminishing the apparent solidity; from which it follows that fraud can be practised, as by legerdemain, without a probability of detection.

This artifice can, however, be defeated by taking off the top, where the diameter is not less than a third part of that at the BUTT.

I refer the reader to Hutton's Demonstrations of these matters, in Bonnycastle's Mensuration.

That some plausible argument has been brought by the advocates of the present system in favour of its continuance, is not to be questioned; or certainly any thing so erroneous could not have prevailed so long, where authors of so much erudition and integrity have been so fruitful in their reprobation of it; particularly in a nation where timber is so valuable an article, and where encouragement to the grower is of so much national importance.

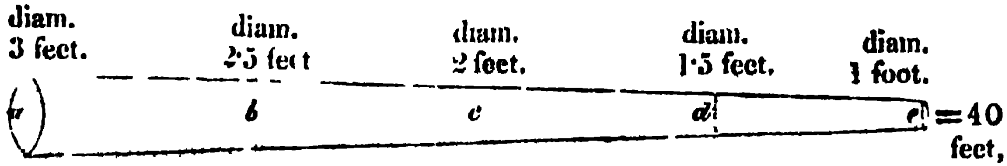
Mr. Bonnycastle observes that the only argument in its favour has been "its ease in practice;" and I therefore trust I shall overcome this argument by submitting a plan scarcely requiring half the time or trouble of the prevailing method; and at the same time obviating a loss to the grower of above 20 *per centum* in any case.

The present practice is, to multiply the length of the log into the square of the fourth part of the girt, taken at the half length: but in real practice the sliding-rule is used; which, in

effect,

effect, is just the same as the foregoing precept; but the operation, upon the sliding-rule is so simple, that the most illiterate feller of timber can perform it just as well as the most expert mathematician; and I am free to confess, that no other method has yet been proposed, as a substitute, by any means so brief; though an approximation given by Mr. Bonnycastle, to find the content of a cylinder, is certainly very simple. He asserts, that "it is as easy in practice as the false method;" but, as regards the sliding-rule in the hands of illiterate mechanics, it certainly is not so.

The method he proposes is, to multiply a fifth part of the middle girt squared into twice the length of the log; and as this would give the content of a cylinder of the same length and of the same diameter as that in the middle, or at the half length, I intend to offer two practical and simple methods, which shall effect the object which he contemplated; and, at the same time that it secures to the grower in any case *above* 20 *per centum* nearer to the value of his property, still leaves an ample excess of quantity to compensate for irregularities of growth to the purchaser; because the real quantity, as the frustum of a cone, will ever be in excess of the quantity resulting from the diameter in the middle, taken as a mean for a cylinder, as will plainly appear from the following:



Suppose the foregoing figure to represent a log of round timber of 40 feet in length.

The greater diameter at $a = 3$ feet;

The middle diameter at $c = 2$ feet; and

The least diameter at $e = 1$ foot.

The content of such a log is truly 136 feet, and 136 thousands of another foot; see the following computation, viz.

$3 \times 1 \times 3 = 9 =$ tripled product of extreme diameters.

$3 - 1 = 2^2 = \underline{4} =$ squared difference of those diameters.

Sum $= 13 \times {\cdot}7854 \times \frac{40}{3} = 136{\cdot}136$ feet.

Now, if this log be measured by the present customary method, one-fourth of the girt at *c* is $= 1{\cdot}5708$, which squared is 2·4674, which multiplied into the length (40 feet) gives 98·7 feet very nearly; and if we take

98·7 feet (the customary content) from

136·136 feet (the real content), there remain

37·436 feet (the loss sustained by the grower) which is *almost* 27½ *per centum!*

But it may be observed, that the greater the disparity of the bases, the greater the loss to the grower; there being a constant fluctuation; and which, however high, as before observed, is *never so low as 20 per centum.*

I will therefore show two original methods of ascertaining the *cylindrical content* of any log.

First original Method.

1st. Measure the length as usual.

2d. Measure the diameter at the half length with a pair of *sliding-rule callipers*, which I have constructed for this sole purpose. And

3dly. (Upon this new instrument) Set the diameter on C to the same on D; and against the length on A stands the content on B.

The operation for the figure will be,

	on C.		on D.		on A.		on B.	
As	2	:	2	: :	40	:	125·66	= feet in a cylinder.
	diam.		diam.		length.		content.	

And if from 125·66 feet, *the cylindrical content,*
we take 98·70 feet, *the customary content,*

there are left 26·96 feet, gained to the grower by the method here proposed.

And if from 136·136 feet, *the real content,*
be taken 125·660 feet, *the cylindrical content,*

there are left 10·476 feet in favour of the purchaser, as a compensation for irregularities of growth. But where timber is regularly grown, and there is so great a disparity between the extreme diameters as 3 to 1, according to the figure, and the timber be of the more valuable species, the same allowance does not seem just; and in such case the log should be measured in two *frusta* of equal length, thus:

Take the diameter of the larger frustum at *b*, of the figure, or a quarter the length of the entire log, for the one-half length; and at *d*, or three-quarters the length, take the diameter for the other half length, which, to follow the precept before laid down, will give as follows; viz.

	on C.		on D.		on A.		on B.
As	2·5	:	2·5	: :	20	:	98·175 feet.
	diam.		diam.		half length.		content of larger frust.

	on C.		on D.		on A.		on B.
and	1·5	:	1·5	: :	20	:	35·343 feet.
	diam.		diam.		half length.		content of smaller frust.

Here larger frustum = 98·175 feet,
and smaller frustum = 35·343 feet.

133·518 feet, content of both frusta.

And if from the real content, 136·136 feet,
be taken the foregoing content, 133·518 feet,

there are left in favour of the buyer 2·618 feet.

It is worthy of notice that the content, given by the customary method, of this whole log was 98·7 feet; whereas the larger frustum measured conically is *really* 99·485 feet, the latter being for only half the length; and by the foregoing method of cylinders the larger frustum yields 98·175 feet, being only about half a foot less than the whole customary content: hence the *butt half, a, b, c, measured truly*, yields more timber than the *entire log measured in the usual way*!!!

I now proceed to show my

Second original Method.

As an example, I give a copy of a page of my manuscript table, prepared expressly for this purpose.

Diameter, 2 feet.		Diameter, 2 feet.		Diameter, 2 feet.	
Length in feet.	Content in feet. Factor 3·1416	Length in feet.	Content in feet. Factor 3·1416	Length in feet.	Content in feet Factor 3·1416
·1	0·31	9	28·27	26	81·68
·2	0·63	10	31·42	27	84·82
·3	0·94	11	34·56	28	87·96
·4	1·26	12	37·70	29	91·11
·5	1·57	13	40·84	30	94·25
·6	1·88	14	43·94	31	97·39
·7	2·20	15	47·12	32	100·53
·8	2·51	16	50·27	33	103·67
·9	2·83	17	53·41	34	106·81
1	3·14	18	56·55	35	109·96
2	6·28	19	59·69	36	113·10
3	9·42	20	62·83	37	116·24
4	12·57	21	65·97	38	119·38
5	15·71	22	69·12	39	122·52
6	18·85	23	72·26	40	125·66
7	21·99	24	75·40		
8	25·13	25	78·54		

This table exhibits at one view the content for any length of two feet diameter: thus at 40 feet in length stands 125·66 feet, which is that found by the first original method by operation; and as all other diameters are treated in the same manner, in my general table, this sufficiently illustrates the whole.

The *factor* at the head is a common multiplicator to reduce any length of that diameter to the content. Thus:

Factor 3·1416
40 length.
Feet 125·6640 content,

which agrees both with the tabular content at 40 feet length, and also with the content given by the operation.

The argument, hitherto so plausible, of "*ease and brevity*," falls, therefore, at once to the ground; because neither of the original methods here proposed requires either the same time or trouble as the girting method. Even the measuring of the log in two frusta requires no more time than to girt; for it is manifest that the taking of a diameter is not a fourth of the trouble of girting and quartering the girt, and then applying the latter to the rule to determine the real dimension; and in surveying of timber standing, these original methods are not a fourth of the labour of the girting or customary method.

As this cannot be viewed as a trivial matter to many of your readers, I beg, in conclusion, to make them an offer, collectively and individually, of my personal service to carry this theory into absolute practice; and as they value their own interest, and prefer truth to falsehood, I invite them to join with me in exploding a system which has been so long the theme of just reprobation.

An Act for the future regulation of weights and measures is before the Legislature.

Let the precepts here submitted be recognised in that Act, and I pledge myself, that within six months afterwards I will put it into practice in every part of the nation.

I am, gentlemen,
Your most obedient humble servant,
WILLIAM GUTTERIDGE.

LXXXV. *On the Theory of parallel Lines in Geometry.* By *Mr.* HENRY MEIKLE.

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—THE theory of parallels is a subject which seems to have engaged the attention of geometers from a period as remote at least as the days of Euclid down to the present time: but their efforts, however powerful, have usually been exerted to very little purpose; for this difficulty, which Euclid left as an exercise for succeeding geometers, appears to have suffered no change during the lapse of twenty centuries, if it is even now demonstrated.

"There is scarce any thing," says Mr. Thomas Simpson, "more obvious to sense, and at the same time more difficult to demonstrate, than the first and most simple properties of parallel lines." So true is this observation, that the very diagrams themselves seem to refuse being so distorted as to suit the conditions of any supposition contrary to Euclid's 12th axiom, or which denies that the angles of a triangle amount to two right angles: and yet all this distortion, although offensive to the eye, is quite consistent when we attempt to reason on it, and compare its several parts. The reason why in this case we arrive at a consistent conclusion even when proceeding on an erroneous supposition, seems to be, that we are not as yet in possession of any property of lines or angles which can counteract our supposition and lead to a contradictory conclusion, or *reductio ad absurdum*; our supposition itself being the only condition that the investigation involves.

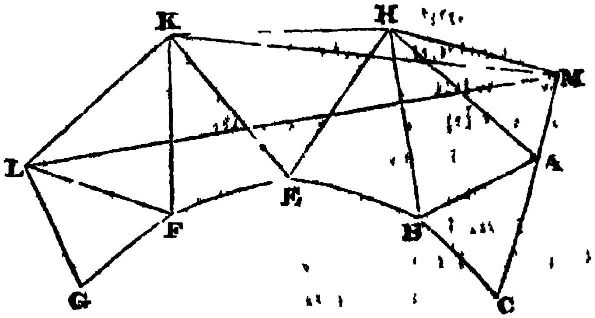
In the Philosophical Magazine for March last, the attention of your mathematical readers has been again directed to this very difficult subject, by your distinguished correspondent Mr. Ivory, who has of late furnished you with so many valuable articles. After a number of instructive preliminary remarks, Mr. I. takes occasion to mention the demonstration of Legendre, but not without pointing out an objection it is liable to, on account of a new principle or axiom which enters into its composition. Mr. Ivory then proposes a demonstration of his own, which, he says, requires no new principles, and is liable to no objection excepting its length. I suppose therefore that every one is perfectly at liberty to state any reasonable doubts or objections he may have regarding the legitimacy of that demonstration.

As to the first part, which is intended to prove that the three angles of a triangle cannot exceed two right angles, there can be no doubt that it is rigidly demonstrated. But I cannot entertain quite so favourable an opinion of the latter part of the performance, because its learned author appears to have overlooked a very important circumstance in the demonstration of his third proposition. This, however, he may still be able to put to rights; but should he fail in doing so, I suppose we may reasonably despair of any other person's giving us an unobjectionable demonstration of the theory of parallels. The defect alluded to will be more readily seen, by extracting the first paragraph and annexing a diagram somewhat different from the original.

"Prop. III. The three angles of a triangle are equal to two right angles."

"If what is affirmed be not true, let the three angles of the triangle A C B be less than two right angles, and let the defect from two right angles be equal to the angle x. Let P stand for a right angle, and find a multiple of the angle r, viz. $m \times r$, such that $4P - mx$, or the excess of four right angles above the multiple angle, shall be less than the sum of the two angles A C B, A B C of the proposed triangle. Produce the side C B, and cut off B E, E F, F G, &c. each equal to B C, so that the whole C G shall contain C B m times; and construct the triangles B H E, E K F, F L G, &c. having their sides equal to the sides of the triangle A C B; and consequently their angles equal to the angles of the same triangle. In C A produced take any point M, and draw H M, K M, L M, &c.; A H, H K, K L, &c."

Having thus given the construction, Mr. Ivory proceeds prematurely with the investigation: for it ought to have been previously demonstrated, that in such a construction the points H, K, &c. lie respectively below the lines K M, L M, &c. This he tacitly assumes without proof; and it is this assumption which stands opposed to the angle x, and enables him to bring out an absurd conclusion; for if no such assumption is ventured on, the investigation, so far as I can see, comes to nothing at all, as is abundantly evident from what follows.



The triangles A B H, H E K, &c. having by construction two sides, and the contained angles in each equal to those in another, are equal: hence the angles A H K, H K L, &c. are equal; and the three angles of each of the triangles A B C, B H E, &c. being less than two right angles by the angle x, the angles A H K, H K L, &c. are each short of two right angles by a quantity not less than x, but perhaps by one much greater. If therefore x be a right angle, each of the angles A H K, H K L cannot exceed a right angle, but, for any thing we are yet supposed to know, they may be far short of right angles; because the triangles A B H, H E K, &c. may each have the sum of its angles much less than two right angles.

If AHK, HKL are right angles, then AH and LK being parallel (28. I. Eucl.) do not meet though produced; much less can the production of LK meet the extension of CA above A; wherefore the point K lies above LM. Also the angle CAH being equal BHK, is less than a right angle; and therefore if AC and HK meet at all, it must be downward; hence the point H lies above KM.

It is no doubt a pretty liberal supposition which makes x a right angle; but by supposing it smaller, the number of the lines HK, KL, &c. is just so much the greater, and the defect of each of the angles AHK, HKL, &c. from two right angles, is so much the oftener repeated. So, if in this case it be difficult to prove that any of the lines HK, KL, &c. do not meet CA above the point A, it is certainly as hard a task to show that they do meet that line, for this is just equivalent to proving Euclid's twelfth axiom. I am, gentlemen, yours, &c.

Sept. 25, 1822. HENRY MEIKLE.

LXXXVI. *On the Autumnal Flowering of the Narcissus.*

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—I FEAR that if florists should adopt the hint given by Dr. Forster in page 343 of the last Number of your Journal, for producing "an autumnal crop of spring flowers," they will in general be disappointed; at least I have reason to believe that the flowering of *Narcissus Tazetta* in his garden in the last month, was not owing to the cause which he assigns. In the garden of a friend of Dr. Forster's near Godstone, he may now see a profusion of fine flowers of both the white and the yellow varieties of *Narcissus Tazetta*, although the same bulbs had flowered well in the spring. The truth is, that the autumnal flowering of this species of Narcissus, when left in the ground, is not so rare a circumstance as Dr. Forster seems to think; and I would submit to his superior judgement, whether its flowering in such abundance this autumn may not be satisfactorily accounted for, when we consider the extraordinary mildness of the last winter, the early spring, the consequent early period at which the bulbs went into a state of rest, the dry summer, and finally, the mildness and moisture of the autumn*. I am, gentlemen, your obedient servant,

Dec. 2, 1822. LE CAYMA.

* We add the following instances.—EDIT.

At the "Glasgow Florist's Club," the following flowers were brought forward in full bloom, viz. Wallflower, Ten-week Stocks, Anemones, Yarrow, Polyanthuses, Gentianella, Carnations, Pinks, crimson Primroses, Auriculas, China Roses, and the Christmas Rose.—*Glasgow Chronicle, Dec.* 14.

Several bird's nests have been found in the parishes of Chilham and Godmersham, with eggs, and the old birds sitting. There is also in the parish of Woreborne, a peartree in blossom.—*Maidstone Journal, Dec.* 14.

LXXXVII. *An Account of a General Survey of the Heavens undertaken at the Königsberg Observatory. By Professor* Bessel *.

The *Histoire Céleste* has enabled us to acquire a more complete knowledge of the firmament than the world had ever before received upon the subject. It has detailed the stars from the north pole to the southern tropic, down to the 8th magnitude inclusive, and even some of a still smaller magnitude, with great accuracy: whilst formerly those only of the telescopic stars were known which had *accidentally* presented themselves to the eye of the astronomer. The observations contained in the *Histoire Céleste* are as accurate as it is reasonable to expect: the places of the stars may be taken from it with sufficient exactness to ascertain every astronomical fact, by uniting it with the second great undertaking of our time, the Catalogue of Piazzi. The *Histoire Céleste* lays the foundation for a new epoch in the science of astronomy, which embraces the knowledge of the firmament by means of its manifold connexion with other branches of knowledge, which henceforth demand the utmost attention from astronomers.

However much the *Histoire Céleste* may have developed the subject, it has by no means precluded further inquiries. It is, on the contrary, necessary that these repeated and desirable observations should be extended to the smaller stars; by which means we may become possessed of a perfect catalogue and chart of all the stars down to the ninth magnitude inclusive.

Repetition is necessary, in order not only to give more accuracy to the determinations, but likewise to obtain a sketch of the proper motions of the stars, and to correct those errors of the pen and the press, which too frequently occur. The extension of the investigation to all stars of the 9th magnitude, is, on the contrary, an arbitrary inquiry, which can and must be accomplished, if we are desirous of rendering more perfect the astronomy of the present age, or of leaving to posterity the means of acquiring a more complete knowledge of the heavens. This perfection is the only desideratum; but it is not by meridian observations alone that this information is to be obtained, since many stars (especially in those regions which are *crowded* with stars) are generally passed over unseen or unnoticed. It is possible, however, to observe sufficient to enable us to mark the remaining ones on the chart from their relative positions estimated with the eye, without any material uncertainty; whereby these at least attain the desired accuracy. If we are desirous of proceeding further, and of knowing with certainty the places of all the stars already designated, the charts must regulate our further observations.

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* From M. Schumacher's *Astronomische Nachrichten*, No. 17.

This is a plan, the execution of which demands the greatest exertions; but which will also lead to the most useful results. If it were pursued, it could not fail to furnish advantageous points of comparison for comets and planets. A single comparison of a star in the heavens with the corresponding one on the chart would discover any contradiction, and would without doubt bring to view many new planets hitherto unknown to us. Finally, the accurate knowledge of the firmament possesses an interest for its own sake, which, to me at least, appears so great, that it need not be heightened by the relation of those other advantages to science to which I have already alluded.

The reasons which have determined me not to place the boundary (which must be established somewhere) at the 8th magnitude, are, first, that many regions of the heavens would then become very barren in those stars whose places were determined: secondly, the hope of discovering new planets, by comparing the heavens with the chart, would be instantly destroyed; since, of the four lately discovered planets, three at least do not reach the 8th degree of magnitude. The reasons against extending the boundary to the 10th magnitude, are, first, the crowding of the chart and of the catalogue, which would be the necessary consequence of it; secondly, the enormous increase of a labour which, even in its most limited extent, already presents an obstacle to be overcome only by the most intense application; and lastly, the difficulty of the observations themselves, which must be made without much illumination of the wires in the telescope.

I have never even had the idea of entirely *completing* this plan; but I have always hoped to *contribute to it* by means of a new and arranged series of observations of the declinations, by zones. I have consequently always endeavoured to procure for myself every assistance to the prosecution of this object. As the liberality of His Prussian Majesty enabled me to furnish the observatory with a large instrument of Reichenbach, I was guided in the choice of it by this object; and I esteem myself fortunate in finding in the construction of the Reichenbach meridian circle, a means of so combining this with all the other objects of astronomical research as to leave nothing on any point to be desired.

This instrument was erected in March 1820; and on the 19th of August 1821, the first zone was observed. The interval of about a year and a half was almost exclusively occupied in a series of observations of the circum-polar stars, in order that the properties of the instrument and the reductions employed in the observations might be determined. I thought it right to let these observations precede, without interrupting the course of them by any others, since they must be very numerous to ensure the desired certainty; and no advantage would have resulted from extending very widely the knowledge of the true method of reduction. In August 1821, however, these preparations, and others of which I shall hereafter speak, were completed, and I possessed in Dr. Argelander an assistant upon whose care and zeal, in that share of the undertaking which fell to his lot, I could safely rely. The unsettled state of the weather, which had indeed been remarkably bad, being no longer an impediment to its commencement, or to its progress, I no longer delayed imparting to my astronomical friends a more accurate account of the new series of observations. The first observations with the new instrument soon showed that the loss of time caused by the reading of the four verniers, and of the level, stands in need of much correction, in order to observe with some degree of accuracy the multitude of stars which crowd through the meridian: even a single vernier and the level cause too much loss on the score of time; and the neglect of the latter too much on that of certainty.

I was therefore obliged to think of a method whereby the certainty might be preserved and the consumption of time diminished. I at length accomplished both by fixing to each pillar of the instrument, a large microscope whose line of vision stands perpendicular to the plane of the circle, and in whose focus the cross wires are moved by a micrometer screw. With these microscopes (on both sides of the instrument) immediate observations may be made of the difference of the zenith or polar distance, and of the zenith or polar distance itself since one of them is known. But as it is not possible to give these microscopes such a wide field of vision, as to enable us to perceive in all cases on *which* line of the circle the cross thread is placed, it was necessary to attach a secondary or assistant arc of 5° of extension (expressly furnished with figures) by means of a clamp to that part of the circle on which the observations are to be made. It was also necessary to contrive an instrument which might mark the boundaries of the zones, and inform the observer, by the stroke of a little hammer, of any excess over these boundaries. M. Frauenhofer has had the goodness to construct these various additions (which, by means of one reading, make up for the five which would otherwise be necessary) with the utmost perfection, and in exact conformity with my plan.

From the readings made by these microscopes it is easy to deduce what the immediate observation of the four verniers and the level would have given for each star. For, the place of the point of commencement of the scale of the microscope is sought by selecting an arbitrary point within the circuit of the zone, not only with the microscope, but also on the verniers, &c. With this place is compared the reading for every star: by which means also the declinations may be found, not by the difference between known stars, but by actual observations. The right ascensions will be given immediately by the fundamental stars, while the position of the instrument, with regard to the meridian, is always most exactly known; so that the observations of the zones are founded upon the same basis as that upon which the other observations of mine depend. In this sole dependence on all the preceding determinations consists, without doubt, one essential advantage of this new mode of observation, the peculiarities of which I will now explain somewhat more in detail.

These observations require two observers, one of whom must attend to the right ascension and to the placing the middle of the horizontal wire upon the stars, whilst the other observes the microscope; the former of these is myself, the latter is Dr. Argelander.

When the survey of a zone is to be made, the hammer is screwed up so as to indicate the boundary of such zone; and the subsidiary arc is so placed that the middle of it may fall on the middle of the zone: the northern and southern boundaries of the zone and the level are then read from the four verniers by the microscope, and the state of the meteorological instruments noted. After these preparations the observer moves the telescope slowly up and down until a star appears in the field of view; the horizontal wire is placed on it; and this being marked by a signal to the second observer, the time of transit is observed by means of one wire, the magnitude and other properties* of the star are noted, and each observer writes down his part of the observation; by which means an arrangement is obtained at once favourable to œconomy of time, and to security from error. In this manner the observations may be continued (unless clouds should intervene) for at least an hour and a half without interruption; and are concluded by a repetition of the readings of the microscope and the meteorological instrument. In case no other observations should prevent, a new zone will be examined after half an hour or an hour; so that three hours of right ascension will be observed regularly every night.

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* Under favourable circumstances the magnifying power of this astonishing telescope (=107) exhibits double stars, of the first class, at the first glance; more are already discovered, as well as double stars of the remaining classes. In an unfavourable atmosphere, however, the smaller ones of the first class may easily pass unobserved: but the stars are often so indistinct that it is impossible to calculate on any discoveries of this kind.

The zones are so arranged that the middle of them falls on some even degree of declination. And in order that no chasm may exist between two contiguous zones, as well as to prevent a repetition of the same observations, I have taken the breadth of the zone somewhat more than two degrees: viz. 2° 12′. At first the magnifying power of 66 was employed; but I soon found out that it was too small for so powerful a telescope, and I therefore took, from the 11th zone onwards, one of 107 magnifying power, which has, in fact, less light, and gives somewhat more satisfactory observations.

In an undertaking of this kind, which requires the application of many years, the greatest œconomy of time is necessary: frequently must some other observation be sacrificed to it, which nevertheless, from the opposition of the planets, neither has yet, nor may be, the case. The number of the zones from the 19th of August to the end of 1821, has however only amounted to 39, and up to this time (17th June 1822) to 89; from which it may be inferred how universally bad the weather has been during that period. These observations (with only two exceptions) fall collectively between −5° and +15° of declination; and seldom extend beyond stars of the 9th magnitude. It will however be necessary to describe somewhat more in detail what kind of stars I mean to class under the 9th magnitude, in order to prevent any differences between different astronomers. Tobias Mayer, Piazzi, and the *Histoire Céleste* appear to agree exactly: but Bradley for the most part denotes the stars by a greater magnitude. Maskelyne, on the other hand, has observed stars to which he attributes the 10th, 11th, and 12th magnitudes; which last, by a scale agreeing with the *Histoire Céleste,* he could hardly have seen in the telescope of his mural quadrant. I have endeavoured to follow the *Histoire Céleste*, and frequently agree with it.

I ascribe the 9th magnitude to all those stars which, with a sufficiently powerful illumination of the wires, I can see well enough to render them difficult to pass unobserved through the field of view, on moving the telescope. Stars of the 9th and 10th magnitudes are more difficult to discover, and for the 10th magnitude, even in my telescope of an aperture of four inches, the light must be so much diminished, that the wires can no longer be seen with any distinctness. What stars I arrange under the different magnitudes will be more clearly seen in the annexed Catalogue: but I shall, for the future, pursue the advice of my respected friend Tralles; and in some well-known region of the heavens (perhaps in that of the Pleiades) shall give a number of stars, which I shall class under the different magnitudes. It would be very gratifying to me, and to many others, if Dr. Olbers, who is so perfectly acquainted with the firmament, would publish his opinion concerning these differences.

Those astronomers for whom this account of the commencement of a series of observations possesses any interest, will rejoice also to see the result of an undertaking which, notwithstanding the unavoidable rapidity of the objects, has attained to exactness. The first is so arranged that, in regions abounding in stars, three stars in a minute, and no more, may, on an average, be observed. If it is desired to observe the right ascension by two wires, (which can only happen with a view to greater accuracy, or when there is some doubt of the first wire,) it becomes scarcely possible to accomplish more than two stars in a minute.

It may now be inquired, what are the results of the observations made with one wire. This question can only be answered by repeated comparisons; since the state of the atmosphere is an important object of consideration. If the stars are indistinct and twinkle much, as is unfortunately very frequently the case, the uncertainty of an observation by a single wire is strikingly increased, as I have found by the comparison of observations made under favourable and unfavourable circumstances.

As, however, in an undertaking of such great extent, commenced under the 55th degree of latitude, we must not be too solicitous about the choice of circumstances, but must take advantage of clear weather when it comes, nothing remains but, by a comparison of very numerous observations, to determine a medium value of probable errors. In order to leave no possibility of the existence of arbitrary decisions, and to point out the probable errors which may occur under actual circumstances, and not under those alone which are peculiarly favourable, I have reduced all the stars (which were observed twice or oftener in the year 1821, since the adoption of the greatest magnifying power) to the beginning of 1825; and have annexed the catalogue of them to this paper: from which it will appear that the probable error of an observation is in

$$Æ = \pm 0,''1548, \quad \text{Decl.} = \pm 1'',013.$$

Bradley's observations give for the former 0″,1426, for the latter 0″,98, both of which are less. I am, however, well satisfied with this result, considering the rapidity with which the observations must have been made; and I believe that this degree of exactness is sufficient for most purposes. The same instrument, when read by the verniers, and used at perfect leisure, gives the probable error of the declination in the neighbourhood of the equator = 0″,76. That it is only about one-third greater according to the observation of the zones, is proved in part by the goodness of my apparatus, but still more conclusively by the extraordinary care of my friend Argelander, *without which the apparatus would have availed as little as other good instruments in bad hands.* The publication of the observations will be yearly superintended. The first 39 observations of zones made in the year 1821 are already in the press. The form which I observed in them is not precisely the original one, which would have occupied too much room; but it would not be difficult to restore the numbers in the form in which they were actually observed, which might in some cases be useful. I give in the first column the magnitude of the star; in the second, the number of wires observed; in the third, the time of observation reduced to the middle wire; in the 4th and 5th, the result given by the subsidiary arc and by the microscope; and finally, in the 6th, the apparent declination: that is, affected with refraction. These latter are obtained when the numbers of the 4th and 5th columns (reduced to degrees, minutes and seconds) are added to the apparent declination of the zero point of the microscope, which is found by the above-mentioned readings and the place of the equator on the instrument. This, however, is determined by double observations of the pole-star; and the corrections which are required on account of the small errors of division in the circle, the bending of the telescope, and the weight of the subsidiary arc and of the hammer, are by these means applied. Lastly, the correction of the time, allowing for the deviation of the instrument from the meridian, is made in each zone for the mean declination. In order to render the use of the rough original observations as easy as possible, I hope, at the same time with the observations, to publish tables of reduction similar to those which I have proposed for the *Histoire Céleste*. I have already calculated these tables for the 39 zones observed in 1821, and have also nearly reduced the 194 stars (the list of which is annexed to this paper) to the year 1825.

This notice may for the present suffice. During the ten months' progress of this laborious undertaking I have ascertained the difficulties which a successive survey of the whole heavens, in the climate of Konigsberg, will have to encounter. The number of years which will pass away before its completion, even on the supposition that it pleases Heaven to preserve me in health and vigour, cannot yet be determined. As, however, it is desirable that it should not be very long delayed, I rejoice to have found Professor Struve of Dorpat, and Dr. Walbeck of Abo, willing to undertake a part of the labour, as soon as they are in possession of the requisite means. Other fellow labourers, provided with equally good and powerful meridian circles, will be very desirable; and I am ready to make the necessary arrangement concerning the choice of a region of the heavens. At a time when an Astronomical Society has been instituted, whose principal object is a minute investigation of the heavens, I think that the hope of seeing this plan realized in its fullest extent cannot be deemed altogether extravagant.

[Professor Bessel has subjoined to this valuable paper a copious list of the stars already observed by him; but which we are obliged to omit for want of room. The appeal which is made, in the last sentence, to the *Astronomical Society of London*, we hope will not be made in vain.—EDIT.]

LXXXVIII. *Some Experiments and Researches on the Saline Contents of Sea-Water, undertaken with a view to correct and improve its Chemical Analysis. By* ALEXANDER MARCET, *M.D. F.R.S. Honorary Professor of Chemistry at Geneva**.

IN a paper on the temperature and saltness of various seas, which the Royal Society did me the honour to publish in their Transactions for the year 1819, I threw out a conjecture, that the sea might contain minute quantities of every substance in nature, which is soluble in water. For the ocean having communication with every part of the earth through the rivers, all of which ultimately pour their waters into it; and soluble substances, even such as are theoretically incompatible with each other, being almost in every instance capable of co-existing in solution, provided the quantities be very minute, I could see no reason why the ocean should not be a general receptacle of all bodies which can be held in solution. And although it will appear from the following account, that I have been unsuccessful in some of my attempts to prove the truth of this conjecture, it may fairly be ascribed either to a want of sufficient accuracy in our present methods of chemical analysis, or of the requisite degree of skill in the operator.

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* From the Transactions of the Philosophical Society, Part I. for 1822.

Some time after the communication to which I have just referred, an extraordinary statement was pointed out to me, upon the authority of Rouelle, a French chemist of the last century, from which it appeared that mercury was contained in sea salt[*]: and I saw soon after in the '*Annales du Musée*,' vol. vii. a paper by the celebrated chemist Proust, who, in a great measure, confirmed that statement, by announcing that he had found traces of mercury in all the specimens of marine acid which he had examined.

Improbable as the fact appeared, I thought it worth while to repeat the experiment, and to take that opportunity of making some collateral researches upon other substances, much more likely than mercury to be discovered in sea-water.

For this purpose I availed myself of the kindness of my friend Mr. John Barry[†], who happened to be in the vicinity of Portsmouth, to supply me with specimens of sea-water, carefully concentrated upon the spot, in vessels of Wedgwood ware, and with scrupulous attention to cleanliness in the process. Accordingly he was so obliging, as not only to send me a quantity of brine evaporated under his own eye, in the manner just mentioned, but he also collected for me a valuable series of specimens from the salt-works near Portsmouth, from all the stages of the process, so as to afford me an opportunity of investigating with accuracy all the chemical circumstances of this interesting branch of national œconomy. Finding myself, however, much pressed by time at this late period of the session, I shall, after briefly adverting to Rouelle's supposed discovery, confine myself in this communication to a few observations which I have made on sea-water itself; keeping out of view, for the present, the topic of salt-making, which, however, I intend to resume at some future period, in a more complete and satisfactory manner.

I first attempted to detect mercury in a specimen of *bay-salt*, such as is obtained in the salt-works near Portsmouth, by spontaneous evaporation. This variety of salt forms large crystals, but is always more or less contaminated by earthy matter, which gives it a dirty appearance. It has, probably, a general resemblance to the French *Sel de Gabelle*, which is more impure still, though, I believe, obtained in a similar manner[‡].

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[*] See *Journal de Médecine*, vol. xlviii. 1777, page 322.

[†] Mr. John Barry, of Plough Court, inventor of a new and valuable process for preparing extracts *in vacuo*, &c.

[‡] The name of *bay-salt* is often applied to foreign as well as British salt, and in general it simply denotes that the salt has been obtained by spontaneous evaporation.

Eight ounces of this salt were put into a coated retort connected with a receiver, and about four ounces of nitrous acid were poured upon it. A pretty brisk action took place, which was further increased by the application of heat; fumes of chlorine were immediately disengaged, and a reddish fluid condensed in the receiver; the heat was continued, and gradually raised in a charcoal fire till no acid or moisture any longer came over; at which time a new emission of red fumes indicated that the nitrate formed in the retort was beginning to part with its acid: minute drops of fused salt soon bedewed the upper part and neck of the retort, so as to be mistaken, at first, for a sublimate. This, however, proved to be almost solely muriate of soda; and on careful examination, it did not appear to contain the smallest atom of corrosive sublimate.

I next dissolved five or six pounds of bay-salt in water, and collected in a filter the insoluble earthy sediment, in which Rouelle stated that the quicksilver was usually found. This sediment being carefully dried, and heated to redness in a coated retort, a white sublimate arose, and condensed on the neck of the retort; but this sublimate proved to be muriate of ammonia, and did not contain the smallest portion of corrosive sublimate or other mercurial salt. This sal-ammoniac, though evidently formed during the distillation from the vegetable and animal matter contained in the sediment, suggested to me the idea of looking for ammonia amongst the contents of sea-water.

I now submitted some *Sel de Gabelle*, which I had procured from Calais for the purpose, to similar experiments, and the sediment, also was carefully examined. The result was essentially the same as with the bay-salt. After adding nitric acid to the salt, the heat was gradually pushed to redness; and when all the moisture was evaporated, a white sublimate appeared, as in the former case, which, in this instance, proved to consist almost entirely of nitrate of soda; but always without the least particle of mercurial salt, and without any muriate of ammonia*.

I therefore think myself justified in concluding that the mercury, which other chemists have detected in sea-salt or its products, must have been introduced there from some local or accidental circumstances.

In experiments upon sea-salt, or in general upon the saline contents of the sea, it is obvious that, in order to exclude sources of error, it is necessary to operate upon pure sea-water, and not upon salts obtained from it by the usual processes in the large way, these being always more or less contaminated by the clay pits in which the evaporation is carried on, by the metallic boilers, or other adventitious causes. I therefore now turned my attention to the sea-water itself, and in particular the perfectly pure and transparent specimen of concentrated brine from the Channel, which I have above mentioned. Mr. Barry procured this water near Bembridge floating light, about two miles N.E. of the eastern extremity of the Isle of Wight, and the evaporation which it had undergone at Portsmouth had only separated from it a quantity of calcareous matter, principally selenite *.

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* In the former experiment the sublimate was principally muriate of soda, owing, no doubt, to the decomposition having been less complete, and the operation less gradually conducted than in the latter experiment.

A few pounds of this water were evaporated nearly to dryness, at a gentle heat, so as to reduce the mother liquor to the smallest possible quantity. This liquor was suffered to drain off, and reserved for experiments, as it is in this fluid that any new ingredients are most likely to be detected.

I had suspected that some nitric salt might be found in sea-water; but in this I was disappointed. The discrimination by the shape of the crystals being in this instance scarcely practicable, the mode which I employed for detecting it, consisted in concentrating the bittern in a glass tube or retort, till it began to deposit solid matter, then adding sulphuric acid and gold-leaf, and boiling the mixture; the gold-leaf was not in the least acted upon, nor was any smell of nitric acid perceived; but on adding the smallest quantity of nitre to the same mixture, the gold was dissolved, and the smell of aqua regia was instantly perceived †.

A portion of the said bittern was next examined by appropriate re-agents with a view to detect any minute quantity of earths or metals, especially alumina, silica, iron and copper, which former inquirers might have overlooked; but I could find no other earth except magnesia: and to my surprise, I did not find in the bittern the least particle of lime; which proves that sea-water contains no muriate of lime, as had been generally supposed. I was equally unsuccessful in my attempts to detect iron or copper, by the most delicate tests.

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* The water, immediately on being raised from the sea, had been allowed to stand a sufficient time to deposit the earthy particles suspended in it, by which means it had become beautifully transparent. 100 pounds of the water produced only three grains of earthy sediment, in which I could discover nothing but carbonate of lime and oxide of iron. It is in this sediment, according to Rouelle, that mercury is to be found. I need hardly say that I could not detect in it the least particle of that metal.

† For this easy and elegant process for detecting nitric acid, a point attended with difficulty, I am indebted to Dr. Wollaston.

In fact, neither alkalies, nor alkaline carbonates, precipitated any other substance from the bittern of sea-water, except magnesia.

The deposit obtained at Portsmouth during the early period of the concentration of the water, being analysed, I found it to consist of selenite, mixed with a little muriate of soda, and a portion of carbonate of lime. The presence of this last substance in sea-water, in a state of perfect solution, being, I believe, a new fact, I neglected no means of establishing it with certainty, an object which was accomplished without difficulty *.

Carbonate of magnesia having been supposed by some chemists to exist in sea-water, I looked for it in the same deposit; but I could not detect the least portion of it by the most delicate tests †.

I next turned my attention to the alkaline salts of sea-water: and here I was more fortunate; as I succeeded in ascertaining beyond a doubt, that sea-water contains ammonia, as it yielded sal-ammoniac by evaporation and sublimation. This result was easily obtained. Some of the bittern being evaporated to dryness in a retort, and a low red heat applied, a white sublimate appeared in the neck of the retort, which proved to be muriate of ammonia. The mode in which this substance was identified was as follows:

1. The sublimate was re-dissolved in water, re-evaporated to dryness, and again sublimed by the heat of a spirit-lamp.

2. This new sublimate being again dissolved, and solution of magnesia and phosphoric acid added, a triple phosphate was formed.

3. On adding caustic potash to the solution, and bringing the mouth of a phial containing muriatic acid close to the vessel, abundant white fumes appeared.

4. The sublimate gave precipitates both with muriate of platina and nitrate of silver ‡.

Sulphate of soda having been mentioned by many chemists, as one of the constituents of sea-water, I endeavoured to ascertain,

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* The deposit was treated with acetic acid, which occasioned an effervescence. The clear liquor being then decanted off, and evaporated to dryness, and alcohol added, acetate of lime was found in the filtered alcoholic liquor.

† Namely, solution of the mass in dilute muriatic acid; precipitation of the lime, and addition of phosphate of ammonia to the filtered liquor.

‡ As it did not enter into my plan, on this occasion, to turn my attention to the estimation of proportions or precise quantities, I have not attempted to estimate exactly the proportion which ammonia bears to the other ingredients of sea-water; but as its presence can easily be shown in 100 grains of the bitter salts, its quantity cannot be extremely minute.

certain, whether or not it existed in it. But all attempts to detect this salt in the bittern by crystallization were fruitless, though great pains were taken for that purpose; and I feel the more confident that there is no sulphate of soda in sea-water, as the presence of this salt, in any but the most minute quantities, would be quite incompatible with our knowledge of chemical affinities. For since there are, co-existing in sea-water, muriate of soda and sulphate of magnesia, it is evident that sulphate of soda would decompose muriate of magnesia, which salt is known to be in sea-water. And again we know, that sea-water contains sulphate of lime and muriate of soda; therefore it cannot contain sulphate of soda; for in that case we should have muriate of lime, which I have shown to be contrary to fact.

The last circumstance which I shall at present notice, relates to the state in which potash exists in sea-water *. Potash is found, by its appropriate re-agents, principally in the bittern; but it is found also among the salts which are separated from it, especially in the latter period of crystallization. By further and repeated evaporation of the bittern, and successive separation of the mother-water remaining after the removal of the crystals formed, various distinct crystals were obtained possessing their characteristic form, namely, prismatic sulphate of magnesia, cubic and star-shaped muriate of soda, and rhombic crystals, quite different from either of the other salts. These crystals, which were evidently portions of an oblique rhombic prism, being carefully separated and washed with water and alcohol, proved to be a triple salt of sulphate of potash and magnesia; a salt so easily analysed, that it would be quite superfluous to relate the particulars of the process.

It now remained to be ascertained, whether potash might not also exist in sea-water in the state of muriate of potash, or of triple muriate of potash and magnesia. That a considerable quantity of potash remains in the bittern, even after the separation of the triple sulphate, is easily ascertained; and by careful evaporation it may be made to crystallize as a triple salt in rhombic crystals; but the constitution of this salt is so delicate, that it is liable to be separated into muriate of potash and muriate of magnesia by water alone; and it is with certainty decomposed by alcohol, which takes up the magnesian muriate, and leaves the other undissolved.

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* It will be recollected, that the presence of potash in sea-water, though announced by myself in the paper on sea-water to which I have before alluded, was Dr. Wollaston's discovery. I have likewise to mention, that the above experiments respecting the state in which it exists, were either made by him or at his suggestion.

From the foregoing observations and experiments it may, therefore, be inferred,

1st. That there is no mercury, or mercurial salt, in the waters of the ocean.

2dly. That sea-water contains no nitrates.

3dly. That it contains sal-ammoniac.

4thly. That it holds carbonate of lime in solution.

5thly. That it contains no muriate of lime.

6thly. That it contains a triple sulphate of magnesia and potash.

Some of these circumstances will, of course, require that former analyses of sea-water, and my own in particular, should be corrected and revised; but this I shall not attempt to do, until I have obtained further and still more precise information on the subject.

Harley-street, June 20, 1822.

LXXXIX. *On the visible Solar and Lunar Eclipses which will happen in the Year 1823, as calculated for Greenwich and Aberdeen; the Elements being calculated from the Tables of M.* DELAMBRE *and M.* BURCKHARDT. *By Mr.* GEORGE INNES *.

[The times are inserted according to civil account, the day being reckoned to begin at midnight.]

To the Editors of the Philosophical Magazine and Journal.

GENTLEMEN,—IN calculating the two lunar eclipses, I have increased the moon's equatorial parallax $\frac{1}{60}$ part for the refraction of the earth's atmosphere. Astronomers seem doubtful how much is to be added to the semidiameter of the shadow of the earth as projected at the distance of the moon; but the quantity must certainly bear some proportion to the parallax.

I send you the elements of the solar eclipse, for the use of such as may wish to make a projection; but from the instant of greatest obscuration falling between 5 and 6 o'clock, it will be found very difficult to determine with sufficient accuracy the distance of the corresponding points on the respective paths.

In reference to this eclipse, it will be interesting to observe whether any visible impression is made on the sun's limb at those places which are situated about the extremity of the penumbra. Such places will be found a little to the west, southwest, and south of London.

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* Communicated by the Author.

The elements of the solar eclipse are as follow:

	D.	H.		
Mean time of ecliptic conjunction at Greenwich	July 8	6	44	46,17
Equation of *mean* to apparent time at conjunction	—		4	29,99
Hence the apparent time of conjunction is	8	6	40	16,18
		°		
Longitude of the sun and moon from true equinox		105	14	14,74
Sun's right ascension		106	32	19,62
—— declination north decreasing . .		22	35	30,6
—— horary motion in longitude . .			2	23,00
———————— right ascension			2	23,93
———————— declination . .	—			16,10
—— semidiameter			15	45,57
—— horizontal parallax				8,65
—— latitude	+			0,03
Horary increase of the equation of time				0,396
Obliquity of the ecliptic		23	27	49,76
Moon's latitude north decreasing . .		1	8	42,77
——— equatorial horizontal parallax		1	1	21,15
——— horizontal semidiameter . .			16	42,12
——— horary motion in longitude at conjunction			37	59,971
——— horary motion in longitude for the hour preceding . . .			38	0,072
——— horary motion for the hour following			37	59,871
——— horary motion in latitude at conjunction	—		3	25,886
——— horary motion in latitude for the hour preceding . . .	—		3	25,610
——— horary motion in latitude for the hour following . . .	—		3	26,162
Angle of the relative orbit with the ecliptic		5	50	12,5
Horary motion of the moon from the sun in the relative orbit .			35	43,78

The following are the results which I have obtained in calculating for Greenwich and Aberdeen.

Calculation for Greenwich, reduced Latitude 51° 17′ 59″,0.

		For the Beginning.		For the Apparent Conjunction.		For the End.	
	D.	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″
Instants assumed ... ... July	8	5 13 15,606	5 14 15,612	5 27 15,698	5 28 15,705	5 40 15,784	5 41 15,790
Sun's longitude	...	105° 10 47,390	105° 10 49,773	105° 11 20,757	105° 11 23,140	105° 11 51,740	105° 11 54,123
—— right ascension	...	106 28 50,921	106 28 53,320	106 29 24,505	106 29 26,904	106 29 55,690	106 29 58,089
Moon's true longitude	...	104 19 8,453	104 19 46,460	104 28 0,331	104 28 38,346	104 36 14,520	104 36 52,526
——— true latitude	...	1 13 37,820	1 13 34,428	1 12 50,355	1 12 46,943	1 12 6,971	1 12 3,575
Right ascension of the meridian	...	4 47 45,0	5 2 47,5	8 18 20,0	8 33 22,5	11 33 52,5	11 48 54,9
Altitude of the nonagesimal ...	...	45 57 58,6	46 3 9,9	47 9 41,7	47 14 44,6	48 14 31,0	48 19 25,9
Longitude of the nonagesimal	...	29 55 44,6	30 6 38,9	32 27 48,2	32 38 37,0	34 47 57,1	34 58 41,8
Parallax in longitude	+	42 26,128	42 27,808	42 45,960	42 47,075	42 57,320	42 57,914
Parallax in latitude	—	42 20,477	42 16,420	41 23,851	41 19,814	40 31,678	40 27,686
Appar. diff. long. of the ☉ and ☽		9 12,809	8 35,505	34,466	2,280	7 20,100	7 56,317
Moon's apparent latitude north	...	31 17,343	31 18,008	31 26,484	31 27,129	31 35,293	31 35,889
——— apparent semidiameter	...	16 45,49		16 46,24		16 46,74	
——— ap. mot. from ☉ in 60″ of time		9,567		36,746		9,088	
Errors from instants assumed (the sign — indicates too early, and + too late)		— 5,982	+ 3,585	—34,466	+2,280	— 6,591	+ 2,497
		The eclipse begins at 5h 13′ 53″,127.		Apparent Conjunction at 5h 28′ 11″,972.		The eclipse ends at 5h 40′ 59″,305.	

Calculation for Aberdeen Observatory. Reduced Lat. 56° 59′ 4″,0. *Long. in time* 8′ 34″,5 W.

	For the Beginning.		For the Apparent Conjunction.		For the End.	
D.	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″	h ′ ″
Instants assumed ... ... July 8	4 58 41,067	4 59 41,073	5 25 41,257	5 26 41,264	5 55 41.442	5 56 41,449
Sun's longitude	105° 10 33,090	105° 10 35,473	105° 11 39,823	105° 11 42,207	105° 12 48,940	105° 12 51,323
—— right ascension	106 28 36,529	106 28 38,927	106 29 43,696	106 29 46,095	106 30 53,262	106 30 55,661
Moon's true longitude	104 15 20,007	104 15 58,033	104 33 4,446	104 33 42,462	104 51 26,668	104 52 4,674
—— true latitude	1 14 2,496	1 13 59,008	1 12 25,627	1 12 22,171	1 10 45,050	1 10 42,517
Right ascension of the meridian ...	1 8 52,5	1 23 55,1	7 55 2,5	8 16 5,0	15 26 15,0	15 41 17,4
Altitude of the nonagesimal ...	40 6 26.0	40 11 29,5	42 19 40,7	42 24 28,0	44 38 53,1	44 43 22,1
Longitude of the nonagesimal ...	32 15 46.4	32 25 46,5	36 43 53,5	36 53 45,0	41 38 15,4	41 48 2,3
Parallax in longitude ... ... +	37 33,074	37 35.080	38 15,301	38 16.378	38 31,961	38 31,980
Parallax in latitude ... ... —	46 36,475	46 32,940	45 1,412	44 57,911	43 19,275	43 15,867
Appar. diff. of long. of the ⊙ and ☽	17 40.009	17 2,360	20.076	16,663	17 9,689	17 45,341
Moon's apparent latitude north ...	27 26.021	27 26,068	27 24.215	27 24.260	27 26,675	27 26,650
—— apparent semidiameter ...	16 45,76		16 46,88		16 47.83	
—— ap. mot. from ⊙ in 60′ of time	20,090		36,739		19.114	
Errors from instants assumed (the sign — indicates too early, and + too late)	— 6,48	+ 13,610	— 20,076	+ 16,663	— 11,288	+ 7,826
	The eclipse begins at 4h 59′ 0″,424.		Apparent Conjunction at 5h 26′ 10″,773.		The eclipse ends at 5h 56′ 16″,871.	

The final results of the calculations are as follow:

GREENWICH.

		Apparent time.			Mean time.		
	D.	H.	′	″	H.	′	″
The eclipse begins, July	8	5	13	53,13	5	18	22,55
Greatest obscuration	...	5	27	20,61	5	31	50,13
Apparent conjunction	...	5	28	11,97	5	32	41,49
End of the eclipse ...	...	5	40	59,31	5	45	28,90

Dig. ′ ″

Digits eclipsed, 0 24 51,16, on the north part of the sun's disc.—The moon will make the first impression on the sun's limb at 28¾° to the left of his zenith.

ABERDEEN.

		Apparent time.			Mean time.		
	D.	H.	′	″	H.	′	″
The eclipse begins, July	8	4	59	0,42	5	3	29,80
Greatest obscuration	...	5	26	10,77	5	30	40,33
Apparent conjunction	...	5	26	14,07	5	30	43,62
End of the eclipse ...	...	5	56	16,87	6	0	46,63

Dig. ′ ″

Digits eclipsed, 1 57 20,56, on the north part of the sun's disc.—The moon will make the first impression on the sun's limb at 6½° to the left* of his zenith.

January 26. Moon eclipsed, partly visible.

	GREENWICH.							ABERDEEN.					
	Apparent time.				Mean time.			Appar. time.			Mean time.		
	D.	H.	′	″	H.	′	″	H.	′	″	H.	′	″
The eclipse begins	26	15	24	46,3	15	37	37,4	15	16	11,8	15	29	2,9
Beginning of total darkness		16	22	15,9	16	35	7,6	16	13	41,4	16	26	33,1
Mooon rises .		16	36	29,2	16	49	21,0	16	13	19,4	16	26	11,2
Eclipticopposition		17	10	43,1	17	23	35,2	17	2	8,6	17	15	0,7
Middle . . .		17	11	29,3	17	24	21,4	17	2	54,8	17	15	46,9
End of total darkness .		18	0	42,7	18	13	35,2	17	52	8,2	18	5	0,7
End of the eclipse		18	58	12,3	19	11	5,3	18	49	37,8	19	2	30,8

Dig. ′ ″

Digits eclipsed 20 49 46,9 from the north side of the earth's shadow.—The moon's centre will pass 4′ 28″,0 north of the centre of the earth's shadow, at the middle of the eclipse.

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* We presume the author means on the *left* side when looking through a telescope which *inverts* the objects.—EDIT.

July 23. Moon eclipsed, partly visible.

	GREENWICH.							ABERDEEN.					
	Apparent time.				Mean time.			App. time.			Mean time.		
	D.	H.	′	″	H.	′	″	H.	′	″	H.	′	″
The eclipse begins	23	1	29	35,8	1	35	38,5	1	21	1,3	1	27	4,0
Beg. of total darkness		2	36	22,6	2	42	25,4	2	27	48,1	2	33	50,0
Middle . . .		3	26	2.0	3	32	4,9	3	17	27,5	3	23	30,4
Ecliptic opposition		3	28	3,4	3	34	6,3	3	19	28,9	3	25	31,8
Moon sets . .		4	1	0,0	4	7	3,0	3	31	54,4	3	37	57,3
End of total darkness		4	15	41,4	4	21	44,4	4	7	6,9	4	13	9,9
End of the eclipse		5	22	28,2	5	28	31,3	5	13	53,7	5	19	56,8

Digits eclipsed 18 Dig. 13′ 22″,1 from the north part of the earth's shadow.—The moon's centre will pass 9′ 10″,9 north of the centre of the earth's shadow at the middle of the eclipse.

Aberdeen, Nov. 15, 1822.

XC. *Some Remarks on Urinary Calculi, and their Chemical Examination.* By JOHN MURRAY, *F.L.S. M.G.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

10th July, 1822.

GENTLEMEN, To the combined genius of Berzelius, Marcet, Henry, Prout, and Brande, we are indebted for almost all we know on the highly interesting subject of urinary calculi.

The existence of calculi composed of *lithate of ammonia*, had been called in question; but Dr. Prout had since ascertained its existence in one case at least—a young person: and to those of immature age it has hence been presumed confined. For my own part I am of opinion *none other exists, except of that description.* I have not been fortunate, at least, to meet with any composed of lithic acid; and when we consider the *solubility of this acid*, I do not think, *prima facie*, that it is reasonable to expect to meet with it *uncombined in a concrete form* in urine.

I have drawn my inferences from the numerous examples that have occurred in my chemical examination of these concretions; and must say, that I never met with one that, being mixed with pure *caustic potassa*, did not yield unequivocal evidence of the escape of ammonia, on bringing a feather dipt in acid near. If the acetic acid was weak, this indication might not appear; but if *muriatic* or *nitric acid* was employed, there then remained no doubt of the presence of ammonia. Now what else could it be but ammonia in this case, even if the odour had not proclaimed its presence? and from whence could the ammoniacal gas arise but from the lithate of ammonia? It is not conceivable that it could be obtained by the simple action of the potassa on any *mucus* supposed to be obtained from the bladder; or to be the result of a synthetic structure of elements resulting from the lithic acid itself, part being decomposed by the action of the potassa, and new modelled in this form. As far as we know, neither can be the source of the ammonia thus presented.

Though Professor Berzelius had stated his having met with silica in the urine, Dr. Prout, in his valuable work on calculous diseases, expresses himself completely sceptical as to the existence of silica in calculi. But M. Thénard, in his very excellent "*Traité de Chimie*," even describes the characters of siliceous calculi. He mentions that they possess the same aspect as those composed of oxalate of lime, perhaps a little less coloured; and that they lose nothing by calcination, and form glass by fusion with potassa.

I had occasion not long since to examine fragments of a calculus passed by a gentleman labouring under this dreadful disease. It had all the *general* characteristics of the ammoniaco-magnesian phosphate or triple calculus. It was *white*, *crystalline*, and disengaged *ammonia* by treatment with caustic potassa, in which it was soluble; nor was it *entirely soluble in sulphuric acid*. The insoluble residue had all the characters of silica, and fused into glass with potassa. The calculus in question, though almost transparent, like that composed of ammoniaco-magnesian phosphate, was *not vitrifiable* at a red heat.

The gentleman in question found considerable pain in passing his urine; unless his common food was composed of bland substances, as barley broth, &c.

His medical attendant was very judiciously administering *muriatic acid*. But unless this was conjoined with such bland vehicles, the sensation was described to me as that which might be supposed to arise from an excoriation of the membranous surface by powdered glass.

The method I adopt in order to obtain a general idea of the constituents of urinary calculi being sufficiently simple, perhaps a succinct detail of it may not be uninteresting to the medical practitioner; seeing the manipulation is so easy, and the chemical skill required so very small.

The cystic oxide and the zanthic oxide calculi are so very rare in occurrence, that for the general practitioner they might be well passed unheeded; *confused crystals* and its *fœtid odour* before the blow-pipe characterize the first of these; and the last is discriminated chiefly by forming a *yellow* compound with nitric acid, instead of a pink one as in the case of lithic acid. That called the fusible calculus is a compound of the ammoniaco-magnesian phosphate, and of the phosphate of lime calculus; and is characterized by its easy fusibility, as its name imports.

A fine tile, or in lieu of it the bottom of a china saucer or plate of glass, will suffice; a feather and glass rod; a spirit lamp, and platinum spoon, with a glass mortar and pestle, form the aggregate of the simple apparatus required: a blow-pipe will be seldom needed.

Pure caustic potassa in concentrated solution, and sulphuric, nitric, muriatic and acetic acids, constitute the amount of the re-agents.

In external and physical characters, 1. The lithic acid calculi are yellowish or reddish yellow, and especially on being wetted; 2. The oxalate of lime (or mulberry) calculus is gray, sometimes deep brown, in undulated layers; 3. The siliceous calculus (which is at any rate rare) wears the same aspect as that of the oxalate of lime; or it may be less coloured; 4. The ammoniaco-magnesian phosphate (or triple) calculus is white, crystalline and semi-transparent; 5. The phosphate of lime calculus is white, opake, and non-crystalline: 6. Dr. Prout has described the lithate of ammonia calculus as being of an ash gray.

A fragment of the calculus to be examined is next reduced to fine powder, and a portion of this powder exposed in a platinum spoon to the heat afforded by the spirit lamp. If it blackens and burns away, leaving no residuum, I conclude it to be wholly compounded of lithic acid, or lithate of ammonia. A further portion of the comminuted powder is then mixed with concentrated solution of caustic potassa; and if visible vapours are developed, on the approach of a feather dipt in muriatic acid, it may be concluded that it is composed of lithate of ammonia.

If, instead of the previous indication, it loses apparently nothing by calculation, and after being thus intensely ignited does not vitrify, I conclude it to be, either the oxalate of lime calculus, or that of the phosphate of lime. If it is vitrified at a red heat, then it may be inferred to be the triple calculus; and if it fuses, the fusible one. In order to distinguish whether it be the mulberry calculus (oxalate of lime), or that of the phosphate of lime, bring a drop of acetic acid in contact after calcination, when cold: if it effervesces, it is the oxalate of lime calculus. It may, however, still be so, and not effervesce: a little of the powder should therefore, after calcination, be thrown into water. If lime-water be formed, it will become turbid by blowing through it by means of a quill: in this case, too, the calculus will be that of oxalate of lime. It may be merely added, if that under examination be the triple calculus, it will yield volatile ammonia, on treatment with caustic potassa. I need not particularize; and these are the *general* indices.

I always am, with the highest respect, gentlemen,
Your very faithful and obedient servant,
J. Murray.

P.S.—I notice a slight error in my paper on "Solution developed from the Action of Sulphuric Acid on Chlorate of Potassa in Water." *It dissolves gold leaf very readily*; the foil appearing finally a very thin perforated film having lost all its metalline appearance. Blue colours are promptly discharged. Sodium does not kindle when projected on its surface, though potassium inflames very readily. J. M.

XCI. *Note of some Experiments on the Vapour of Sulphuric Ether.* By [illegible] Murray, *F.L.S. M.W.S. &c. &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,—I am not aware that the following phænomena are any where recorded; many of the experiments detailed are, perhaps, sufficiently curious and interesting to merit a place in your Journal.

The copious and continued extrication of inflammable vapour from the surface of ether is remarkable, and may be made subservient to a chain of experiments illustrative of its physical and chemical character.

The vapour may be poured in an invisible form from one vessel to another, as in the case of carbonic acid gas. An ignited taper will be the test of its presence. In like manner the vapour may be laved or pumped out into other vessels, or transferred by a wide stop-cock laterally, or through a funnel.

The vapour, however, being poured into a glass funnel with a stem eight inches long and the diameter of the bore 1-8th to 1-16th inch diameter, did not flow out, but remained in the funnel above.

The sulphuric ether vapour poured on carbonic acid gas, mingles with it, and from this uniform diffusion, the gas, &c. burn with a diluted blue flame to the bottom of the cylinder containing them.

Mixed with hydrogen, it burns with a lambent blue flame.

With nitrous gas, ignition does not produce explosion. It burns tranquilly and in all respects like a mixture of nitrous gas and hydrogen, having an increased illuminating power, and of a green colour.

Equal parts of the ethereal vapour and nitrous oxide burnt with a flame resembling the last in colour, but the ignition was rapid and approximating the explosive range.

The vapour does not seem to diffuse in olefiant gas,—this last burning with its usual illuminating effect, and it is succeeded by a blueish flame, such a one as seemed to be characteristic of the vapour.

The ethereal vapour poured on chlorine gas in day light, gives rise to the immediate formation of clouds of muriatic acid gas, accompanied by the exhalation of a kind of *spray* from the bottom of the vessel.

Equal volumes of sulphuric ether vapour and ammoniacal gas burn tranquilly to the end, with reddish green flame.

Equal volumes of vapour and cyanogen diluted the flame, and made it apparently more luminous.

Equal parts by bulk of the vapour and sulphuretted hydrogen burn with a lambent blue flame, little sulphur is deposited, and the combustion gives rise to the odour of garlic.

It inflames on the surface of naphtha, water, &c. Poured on the surface of nitric and muriatic acids, it burns with slight alteration of tint.

On ammonia in solution the flame has a reddish tint. The combustion from the surface of sulphuric acid is more rapid, somewhat of an explosive kind, and the flame is of a red cast.

Oct. 7, 1822. J. MURRAY.

P.S. *Gold-leaf* was immersed in the vapour of sulphuric ether, and allowed to repose for four weeks, without any perceptible change having been superinduced. Thin slips of caoutchouc exhibited no alteration whatever in eight days: a small quantity of *distilled water* was at the termination of this period added. In some days the slips were slightly *blanched*, the edges being most whitened. In the change here produced there seemed an approximation to the consistency of *leather*.

I shall only take leave to add, that the thin slips of caoutchouc referred to were obtained by inflating, by means of a condenser, small bottles of Indian rubber in the way described by Mr. Forster.—I have thus expanded bottles of caoutchouc, from the size of a walnut to the diameter of six inches and more. I have found strips of the caoutchouc so extended, *admirable valves* for the condenser of my gas blowpipe."

XCII. *True apparent Right Ascension of Dr.* MASKELYNE'S 36 *Stars for every Day in the Year* 1823, *at the time of passing the Meridian of Greenwich. Calculated from* BESSEL'S *Tables of* 1820.

1823.	γ Pegasi.	α Arietis.	α Ceti.	Aldebaran.	Capella.	Rigel.	β Tauri.	α Orionis.	Sirius.	Castor.	Procyon.	Pollux.	α Hydræ.	Regulus.	β Leonis.	β Virginis.	Spica Virginis.	Arcturus.
	H. M. 0 4	H. M. 1 57	H. M. 2 53	H. M. 4 25	H. M. 5 3	H. M. 5 6	H. M. 5 15	H. M. 5 45	H. M. 6 37	H. M. 7 23	H. M. 7 30	H. M. 7 34	H. M. 9 18	H. M. 9 58	H. M. 11 40	H. M. 11 41	H. M. 13 15	H. M. 14 7
Jan.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	8·60	14·32	3·89	48·49	40·56	4·11	9·02	37·62	22·83	19·91	4·11	30·85	54·99	57·97	2·79	29·80	53·42	35·84
2	59	31	89	49	56	11	02	63	84	93	12	86	02	58·00	83	83	45	88
3	58	30	88	49	56	11	03	63	84	95	13	88	04	02	86	86	49	91
4	57	29	87	49	56	11	03	64	85	96	15	90	06	05	89	90	52	94
5	56	28	86	48	56	11	03	64	86	98	16	92	09	08	92	93	55	97
6	55	27	86	48	56	11	04	65	87	99	17	93	11	10	96	95	59	36 01
7	54	25	85	48	56	11	04	65	87	20 01	19	95	13	13	99	99	62	04
8	53	24	84	47	56	11	04	65	88	03	20	96	15	16	3·02	30·03	65	07
9	52	23	83	47	56	11	04	65	88	05	21	98	18	18	05	06	69	10
10	51	22	82	47	56	11	04	66	89	06	23	31·00	20	21	09	09	72	14
11	50	21	81	46	56	10	04	66	89	07	24	01	22	24	13	12	75	17
12	49	19	80	46	55	10	04	66	90	08	25	02	24	26	15	15	79	21
13	48	18	79	46	55	10	04	66	90	09	27	04	26	28	18	18	82	24
14	47	17	78	45	54	09	04	66	90	10	28	05	27	31	21	21	85	27
15	46	16	77	45	54	09	03	66	90	11	29	06	29	33	24	24	88	31
16	45	14	76	44	53	08	03	66	91	13	30	07	31	35	27	27	92	34
17	44	13	75	43	53	08	03	66	91	14	31	08	33	38	30	30	95	37
18	43	12	74	43	52	07	02	66	91	15	32	09	35	40	33	33	98	40
19	42	11	73	42	51	07	02	66	91	16	33	11	37	43	36	36	54·02	44
20	41	09	71	41	50	06	01	66	91	16	33	12	39	45	39	39	05	47
21	40	08	70	41	49	05	00	66	91	17	34	12	40	47	42	42	08	51
22	39	06	68	40	48	05	00	65	91	18	34	13	42	49	45	44	11	54
23	38	05	67	39	47	04	8·99	65	91	18	35	14	43	51	47	47	15	57
24	37	04	66	38	46	03	98	65	90	19	35	14	44	53	50	49	18	60
25	36	02	65	37	45	02	97	64	90	19	36	15	46	55	52	52	21	64
26	35	01	63	36	44	01	97	64	90	20	36	15	47	57	55	55	24	67
27	35	00	62	35	43	00	96	63	89	20	36	16	48	58	58	57	27	70
28	34	13·98	61	34	42	3·99	95	63	89	21	37	17	50	60	60	60	30	73
29	33	97	60	33	41	98	94	62	89	21	37	17	51	62	63	63	34	77
30	32	96	58	32	39	97	94	62	88	21	37	17	53	64	66	65	37	80
31	31	94	57	30	37	96	93	61	88	21	37	17	54	66	69	68	40	83

1823.	1 α Libræ.	2 α Libræ.	α Cor. Bor.	α Serpentis	Antares.	α Herculis.	α Ophiuchi.	α Lyræ.	γ Aquilæ.	α Aquilæ.	β Aquilæ.	1 α Capri.	2 α Capri	α Cygni.	α Aqua.	Fomalhaut.	α Pegasi.	α Andromedæ.
	H. M. 14 40	H. M. 14 41	H. M. 15 27	H. M. 15 35	H. M. 16 18	H. M. 17 6	H. M. 17 26	H. M. 18 30	H. M. 19 37	H. M. 19 42	H. M. 19 46	H. M. 20 7	H. M. 20 8	H. M. 20 35	H. M. 21 56	H. M. 22 47	H. M. 22 55	H. M. 23 59
Jan.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.	s.
1	55·04	6·46	11·67	33·33	34·05	34·39	42·76	55·63	50·16	8·29	36·68	49·57	13·39	22·76	41·46	51·50	57·26	15·88
2	08	50	70	36	08	41	78	64	17	30	69	57	39	75	45	49	25	87
3	11	53	73	39	11	43	80	65	18	31	70	57	39	75	45	48	24	86
4	14	56	76	42	14	45	82	66	18	31	70	58	40	74	44	47	23	85
5	18	60	79	45	17	47	84	67	19	32	71	58	40	74	44	46	22	83
6	21	63	82	48	20	49	86	69	20	33	72	59	41	73	43	45	22	82
7	24	66	85	51	23	51	88	70	20	33	72	60	42	73	43	44	21	81
8	27	69	88	54	26	53	90	71	21	34	73	60	42	72	43	43	20	79
9	30	72	91	57	29	55	92	72	22	35	74	61	43	72	42	42	19	78
10	34	76	94	60	32	58	94	74	23	36	75	62	44	72	42	42	19	77
11	37	79	97	63	35	60	96	76	23	37	76	63	45	71	42	41	18	75
12	41	83	12·00	66	38	63	98	77	24	38	77	63	45	71	42	40	17	74
13	44	86	03	69	41	65	43·01	79	* 25	39	78	64	46	71	41	40	17	73
14	48	90	06	72	44	68	03	81	27	* 40	79	65	47	71	41	39	16	72
15	51	93	09	75	47	70	05	83	28	42	* 80	66	48	71	41	38	15	70
16	54	96	13	78	51	73	07	85	29	43	82	67	49	71	41	38	15	69
17	58	7·00	16	81	54	75	10	86	31	44	83	68	50	71	41	37	14	68
18	61	03	19	84	57	78	12	88	32	45	84	69	51	71	41	37	13	67
19	65	07	22	87	60	80	14	89	33	46	85	70	52	71	41	36	12	66
20	68	10	26	90	64	83	17	91	35	47	86	* 71	* 53	71	41	36	12	65
21	72	14	29	93	67	86	20	93	36	48	87	73	55	71	41	35	11	64
22	75	17	32	96	71	89	22	95	38	49	88	74	56	72	41	35	11	63
23	78	20	35	99	74	91	25	97	39	51	90	75	57	72	41	34	10	62
24	82	24	39	34·02	78	94	28	99	41	52	91	77	59	72	41	34	10	61
25	85	27	42	05	81	97	30	56·01	43	54	93	78	60	73	42	33	10	60
26	89	31	45	08	85	99	33	03	44	55	94	80	62	* 73	42	33	09	59
27	92	34	49	11	88	35·02	35	05	46	57	96	81	63	74	42	33	09	58
28	95	37	52	14	92	04	38	07	47	58	97	83	65	75	42	32	09	57
29	98	40	55	17	95	07	40	09	49	60	99	84	66	76	42	32	08	56
30	56·02	44	58	21	99	10	43	12	50	61	37·00	86	68	77	43	32	08	55
31	05	47	62	24	35·02	12	46	14	51	63	01	88	70	78	43	32	08	54

XCIII. *On the Expansive Force of Steam at different Temperatures.* *By* PHILIP TAYLOR.

To the Editors of the Philosophical Magazine and Journal.

Bromley, Middlesex, Dec. 26, 1822.

GENTLEMEN,—MUCH interesting discussion having lately taken place on the expansibility of steam, I shall feel pleasure if I can in any degree contribute toward a more correct knowledge of the physical properties of this important agent.

Some years since, I proposed a safe and simple mode by which the heat of compressed steam might be employed, to any extent, in various manufacturing processes.

In this application of steam, it became necessary to determine what precise temperature would result from its different degrees of compression; for in many cases it was desirable to employ it as the medium for conveying heat to solutions of saline substances and sugar, which require high degrees of heat for their ebullition.

Finding that my practical results did not accord either with the experiments given by other writers, or with the theories deduced from them, I was led to form a Scale (Plate VI.) which might show the expansive force consequent on each degree of temperature, from 212° to 320° Fahr.

As this Scale was intended to exhibit the force of steam *above* the point at which the atmospheric pressure balances it, I have placed my zero at 212°, and given the forces produced by each addition of temperature in inches of mercury; and in order that it might be rendered more convenient for use, the divisions are reduced to one-fourth of their actual magnitude. I have also in another column given the pressure on a square inch at each degree of temperature—assuming that $2\frac{1}{10}$ inches of mercury of sp. gr. 13·50, are equal to one pound per square inch.

The rapidly increasing series of the forces compared with the regular increments of heat, is at one view exhibited on this Scale, in a manner which I hope will be interesting to the man of science, and useful to those who are like myself engaged in the practical application of the force and heat of steam.

This Scale was formed by the means of an apparatus likely to render my experiments as correct as any can be, which depend on the indications of thermometers; but I have hitherto hesitated in publishing it, from its great variance with those Tables already given by men of science.

Repeated experiments having led me to believe that my results are correct, I send it for your insertion:—at the same time I wish to observe, that I have not endeavoured to make it otherwise than what I profess it to be, a *practical scale*, and I am aware that many little discrepancies will be found, which are not to be reconciled with theory.

As it may be useful to bring under one view the forces, in inches of mercury laid down by others, and compared with my own, I subjoin them:

Temperat.	Robison.	Watt.	Southern.	Dalton.	Ure.	Taylor.
220	5·8	4	....	4·99	5·54	4·95
230	14·5	10·5	....	11·75	13·10	11·51
240	24·9	19	....	19·67	21·70	20
250	36·8	28	30	28·21	31·90	29·12
260	50·3	37	....	37·73	42·30	40·10
270	64·1	49	....	47·85	56·30	52·50
280	75·9	....	....	58·75	71·90	67·75
290	....	....	....	70·12	90·15	84·50
293·4	....	....	90	....	....	90·40
300	....	....	....	81·81	109·70	103·75
320	....	....	....	105	....	149·40

The experiments of Dr. Robison and Mr. Watt appear only to have been carried to 270° and 280° of temperature, and this may probably account for some conclusions drawn from them which appear to me erroneous.

The forces of steam above 212° given by Mr. Dalton, were, I believe, derived by calculation from the force of the vapour of water below the boiling point; in this case, if incorrect data were assumed, errors magnified as the series advanced.*

The two points given by Mr. Southern from the experiments tried for the Royal Society, coincide very nearly with my own; and I regret that the series was not rendered complete by that gentleman.

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* On n'a pas poussé cette table (calculée d'après la formule que j'ai déduite des expériences de M. Dalton) au-delà de 130°. parcequ'elle aurait pu devenir fautive. En effet, de pareilles formules ne sont jamais que des approximations, dans lesquelles on ne comprend que les termes qui sont sensibles avec les expériences que l'on compare. Il ne faut donc pas les transporter indiscrètement à d'autres limites plus éloignées que ces observations ne comprendraient pas, puisque les termes négligés pourraient alors acquérir une influence qu'on ne leur avait pas reconnue, et leur absence occasionnerait de grandes erreurs. Ici, par ex. si l'on voulait pousser la formule jusqu'à 200°, on trouverait que la force élastique cesse d'augmenter, et même finit par décroître. Mais cela signifie seulement, qu'en satisfaisant aux premières observations, on a négligé des termes auxquels il faudrait avoir égard pour s'élever à de si hauts degrés. On remédierait à ce défaut, si l'on avait des tensions observées vers ces degrés là; car on y plierait la formule en y ajoutant un terme en n^4. qui serait insensible dans les degrés inférieurs. Mais à défaut de pareilles observations nous avons borné la formule à l'étendue que comportent les expériences de M. Dalton.—Biot, *Traité de Phys. Exper.* t. I. p. 530.

Dr. Ure's numbers are uniformly higher than those which I have given; and as he laid them down by experiment, I suspect that our mode of operating must have differed.

I shall not at present enter upon the question, whether it is more œconomical to obtain power by heating water to a high temperature, or a comparatively low one; for although the annexed Table shows that 38 degrees of heat only produce a force equal to one atmosphere, while about 12 degrees of heat will give the force of the 5th atmosphere; still this fact alone would not prove the œconomy of high-pressure steam.—To settle this question, it is necessary to ascertain what relative proportions of *fuel* must be *consumed* to produce each degree of force.

From experiments made on a large scale, I have long been convinced of the value and œconomy of high-pressure steam,—and this conviction led me to give the opinion which I stated before a Committee of the House of Commons some years since*.

Many important improvements have resulted from the use of this valuable agent since that period; and others, which will arise from our more perfect acquaintance with its properties, will lead us to rejoice that the Legislature did not limit its use in consequence of a few injudicious applications of it.

I am at this time engaged in experiments to ascertain at what expense of fuel each degree of force can be obtained; and I have reason to think that the apparatus which I have constructed will give me results on which I may rely.

I am, Gentlemen, yours truly,

PHILIP TAYLOR.

P.S. I observe that the engraved plate varies a little from the original, owing to the shrinking of the paper on drying;—but as this effect is equal, it does not interfere with its comparative accuracy.

XCIV. *Notices respecting New Books.*

Conversations on Mineralogy: with Plates engraved by Mr. and Miss Lowry, from Original Drawings. 2 vols. 12mo.

THIS work, though its pretensions are modest and unassuming, will be found highly useful, and well adapted to answer the purpose for which it is intended, namely, as stated by Miss Lowry in her preface, for it is to this lady that we are indebted for the publication, "to *prepare* the young mineralogist for the study of more learned treatises."

The conversational method of imparting knowledge appears to be well adapted for elementary books, as the difficulties most likely to occur to a beginner are stated and answered,

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* Phil. Mag. Vol. L. p. 169.

swered, and thus leave a stronger impression on the mind than a simple affirmation of the facts. Of the sixteen conversations which the book contains, the first is introductory, containing definitions of mineralogy and geology; their relation; the connexion of mineralogy with chemistry; and a list of the simple or elementary substances of which minerals are composed. In the second and third the properties and appearance of these substances, and their comparative utility to mankind, in a simple or prepared state, are explained as fully as is consistent with the nature of the work: in the third conversation an explanation is also given of the term specific gravity, and the manner of determining it. The fourth conversation is an introduction to the mathematical part of mineralogy, crystallography; the theory of Haüy is here adopted, as the most capable of explaining the internal structure of crystals consistently with their external transitions of form: the system of Professor Mohs is not capable of being adopted, or even properly explained, in a work of this extent, and it is, besides, not generally known in England. The uses of the goniometer, electrometer, magnet, blowpipe, and chemical tests, with the principal external characters of minerals, are explained in the succeeding conversation; and in the sixth are some remarks on the classification of minerals, and a tabular sketch of the one adopted. Miss Lowry's arrangement is new: though founded, like most others, on a combination of the chemical and physical characters of minerals, it is made to depend, in some degree, on their comparative age: thus, the earthy compounds being more ancient than the metalliferous ones, *precede* them; and the inflammable substances, which are of more recent formation, are placed *after* them. We observe that she has made an addition of one substance, arseniate of iron, to the list given by Haüy of minerals which are electric by heat. This property of the arseniate of iron has not, we believe, before been remarked by any author.

This work is highly creditable to Miss Lowry, and we cannot doubt will be well received by the public. The Plates, which are twelve in number, are all from drawings by Miss Lowry, executed with a degree of neatness and accuracy which have never been surpassed.

The Linnean Society have just published the Second Part of the 13th Volume of their Transactions, containing the following Papers:

Second Part of the Descriptive Catalogue of a Zoological Collection made in the Island of Sumatra and its Vicinity. By Sir Thomas Stamford Raffles.—A Monograph of the

Genus Saxifraga. By Mr. David Don.—On a Fossil Shell of a fibrous Structure, the Fragments of which occur abundantly in the Chalk Strata and in the Flints accompanying it. By Mr. James Sowerby.—Remarks on Hypnum recognitum, and on several new Species of Roscoea. By Sir James Edward Smith.—Remarks on the Genera Orbicula and Crania of Lamarck, with Descriptions of two Species of each Genus; and some Observations proving the Patella distorta of Montagu to be a Species of Crania. By Mr. George Brettingham Sowerby.—A Commentary on the Hortus Malabaricus, Part I. By Francis Hamilton, M.D.—Observations on the Chrysanthemum Indicum of Linnæus. By Joseph Sabine, Esq.—Account of the Marmots of North America hitherto known, with Notices and Descriptions of three new Species. By Joseph Sabine, Esq.—On certain Species of Carduus and Cnicus which appear to be dioecious.—By Thomas Smith, Esq.—The Natural History of Lamia Amputator of Fabricius. By the Rev. L. Guilding.—Description of two new Genera of Plants from Nepal. By Dr. N. Wallich,—Extracts from the Minute-Book, &c.

Recent Publications.

An accurate Table of the Population of the British Empire in 1821; specifying all the Cities and Boroughs in Great Britain, with every other Parish or Place containing 2000 Inhabitants or upwards, &c. On double Demy Paper. Price 5*s.*; fine Paper 7*s.* Published by E. Wilson, Royal Exchange.

A Letter to the Rev. T. R. Malthus, being an Answer to the Criticism on Mr. Godwin's work on Population, in the Edinburgh Review. With an Examination of the Censuses of Great Britain and Ireland. By David Booth.

Preparing for Publication.

In the course of next month (January) will be published, Bohn's Bibliographical, Analytical and Descriptive Catalogue of Books in all Languages and Classes of Literature, including an extensive Collection of Botanical and Scientific Works.

We understand that Francis Maseres, Esq. Cursitor Baron of the Exchequer, whose liberal exertions for the restoration of the older mathematical writers are so well known to the mathematical world, has nearly completed a collection of those which relate to optical science. Amongst the interesting treatises which are reprinted in this volume, are the *Optica promota* of James Gregory, containing the first publication of the reflecting telescope; the *Traité de la Lumière* of Huygens; and the *Lectiones Opticæ* of Dr. Barrow, a work which has become exceedingly scarce. This work is edited under the superintendance of C. Babbage, Esq. F.R.S. &c.

ANALYSIS OF PERIODICAL WORKS ON ZOOLOGY AND BOTANY.

Swainson's Zoological Illustrations. No. 27.

Pl. 130 and 131 illustrate, in the most complete manner, that remarkable bird *Trochilus latipennis*, or Broad-shafted Humming Bird of Latham, which is here called the Gray sickle-winged Humming Bird, for the sake of distinguishing it from two other species, *both with broad shafts* to their wings, first described by Mr. Swainson in this work: both sexes are figured; from which it appears the quill-feathers in the female are similar to those usual in the birds of this genus. Pl. 132. *Macroglossum annulosum* and *fasciatum*, two new insects from Brazil, thus characterized: *M. annulosum*; alis nigris, anticis fasciis 2 hyalinè maculatis ornatis; abdominis nigri, segmento tertio niveo. *M. fasciatum*, alis nigricantibus, anticis fusco variis, posticis strigâ aurantiacâ centrali ornatis, thorace griseâ; corporis lateribus, maculis aurantiacis, nigris et pallidè fulvis insignibus: antennis gracilibus unco producto. Pl. 133. Another new and interesting lepidopterous insect, likewise from Brazil. *Thecla macaria*, with figures of both the sexes; T. alis supra fuscis, anticis ad basin cæruleis, infra ferrugineis, punctis 2 mediis nigris ornatis, posticis infra castaneis, anticè pallidioribus, macula nigra ad basin ornatis Pl. 134 beautifully represents *Strombus lentiginosus*, and a new species by the name of *erustus*, nearly approaching to it, but thoroughly distinguished by these characters: "S. testâ nodosâ, labio interiore albo, lævi; labii exterioris inflexi, supra sinuati, intus purpureo-atri, striati, lobo basali edentulo."

Sowerby's Genera of Shells. No. 9.

Two Plates accompany the description of the genus SPONDYLUS, of which the definition (although apparently correct) is loaded with unnecessary detail, occupying nearly half a page, although its essential characters might have been contained in eight or ten lines. AMPHIDESMA, a genus possessing at the best but dubious characters. SUCCINEA of Draparnaud, accompanied by excellent figures. Mr. Sowerby judiciously adopts this genus after the best continental conchologists, in preference to M. de Ferrussac. Indeed we have been greatly disappointed in the systematic arrangement of the land shells proposed by this last-mentioned author. With facilities for investigating them in the most perfect manner, we had expected something beyond a mere artificial arrangement of divisions "ten times divided," and a whole list of new family names, more perplexing than the genealogy of an extinct dukedom. VULSELLA, with an excellent figure of *V. lingulata.* COLUMBELLA. The plate illustrating this genus is rich in well-drawn figures of no less than nine species of these pretty little shells.

We conclude this article with melancholy regret. After long struggling against an acute and fatal disorder, Mr. James Sowerby has sunk into the grave. As one of the best delineators of natural productions this country has ever produced, his numerous and valuable works through a long life, at once bespeak his excellence and his industry. Science will mourn the loss of one whose labours have contributed in this country so much to her advancement. Indeed, we know not of one who, uniting the professional artist with the scientific naturalist, will supply the gap thus left in the small circle of our native zoological artists. As a private character, we have ever heard Mr. Sowerby spoken of with esteem and respect; and we trust that the many works his unfortunate death has left in an unfinished state, will be completed, either by his sons, or by some other competent person.

Greville's Scottish Cryptogamic Flora. Nos. 5 and 6.

These Numbers of Mr. Greville's interesting work contain the following subjects:

No. 5. Tab. 21. *Erineum Betulæ* Decand. No. 22. *E. pyrinum* Pers. No. 23. *Agaricus tuberosus* Bull.; a most remarkable species. having, as its name implies, tuberous roots, which are attached by means of a few fibres to moss, dead leaves, and black and decayed *Agarics* of other species. No. 24. *Hysterium Rubi* of Pers. And No. 25. *Echinella paradoxa*, a marine production which had never been noticed by any preceding author, except Lyngbye in his excellent *Tent. Hydrophyt. Daniæ.*

No. 6. Tab. 26. *Hysterium Juniperi* of Mr. Greville, a new species found in the vicinity of Edinburgh, as well as upon the highest of the Grampian Mountains, on the dead leaves of *Juniperus communis:* "ovale nitidum subplanum, minutum, cellulis sporuliferis apicibus attenuatis." No. 27. *Cylindrosporum concentricum*, a new genus of plants of Mr. Greville's Division of Fungi, which he calls *Fusidoideæ* (*Epiphytæ*, Link), and thus characterized: "Plantæ minutissimæ in foliis vivis parasiticæ, non rupta epidermide. Sporidia cylindrica truncata, non septata, nuda, libera, coacervata," Grev. There is only one species yet discovered, and that forms spots of white concentric lines on both the surfaces of the leaves of the common Cabbage. Tab. 28. *Agaricus odorus* Bull. et Sowerby, which gives out a scent like that of the *Anthoxanthum odoratum*, or of the Woodruff. Tab. 29. *Puccinia Fabæ*, a new species of the genus found on the leaves of the common Bean, and thus distinguished: " bifrons nigra, depressa, orbiculata. Sporidia 1-lòculata, ovato-globosa, pedicellis elongatis, gracilibus, albis," Grev. Tab. 30. *Gloionema apiculatum*, a new species of marinè *Algæ*, at least only previously described by Mr. Greville in the Transactions of the Wernerian Society of Edinburgh. The genus is invented by Agardh, and has for its character: " Fila gelatinosa, tenacia, continua, intus granulis ellipticis vel cylindricis longitudinaliter farcta." The SPEC.CHAR. "fronde continua, filiforme, ramosa, aliquando fasciculata; granulis cylindraceo-oblongis apicibus ramulorum incrassatis apiculatis." Grev.

Another British species of the genus is the *Conferva fœtida* of Dillw. (*Gloionema fœtidum* Hook. Fl. Scot.)

The Botanical Register. No. 94.

Plate 669 represents *Polygala myrtifolia*, and Pl. 670. *Spathelia simplex*, two Linnæan species long cultivated in our gardens. Pl. 671. *Melastoma granulosa*, from Brazil, a country abounding in these beautiful and very graceful plants. Pl. 672. *Melastoma malabathrica*, from China. Pl. 673. *Passiflora picturata*, a new species, stated to have flowered at Mr. Lee's Nursery last September. This plant was, however, first introduced into this country by Mr. Swainson, who sent it to the Liverpool garden in 1816, from Pernambuco, where it is abundant, though in the southern provinces it has never been found; the fruit, according to Mr. Swainson, is about the size of a small pippin, and most delicious. Pl. 674. *Osbeckia stellata*, a new species from Nepal: " O. foliis lanceolato-oblongis acuminatis 5-nerviis ramisque hispidis, calyce urceolato oblongo muricibus radiato-setosis implexè hirsuto: staminibus adscendentibus, antheris flexuosis filamento longioribus. Pl. 675. *Geodorum dilatatum*: the figure of this plant is objectionable, inasmuch as the artist has drawn it with the spike bent downwards, giving it a pendent appearance quite foreign to the real habit of the plant.

XCV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

ON Saturday, November 30th, being St. Andrew's Day, the Royal Society held their Anniversary Meeting at Somerset-House. A great number of members were in attendance at

twelve

twelve o'clock, when the President Sir H. Davy, Bart. took the chair. In the course of business, in announcing the names of the Fellows lost to the Society during the last year, amongst whom were Sir H. C. Englefield, Sir William Herschel, Dr. Marcet, Professor Vince, Dr. Parry, and M. Delambre; the Abbé Haüy and Count Berthollet on the Foreign list; the learned President gave a new interest to this ceremony by entering into a short detail of the scientific merits of those distinguished persons.

He prefaced his eulogies by saying, that the occasion was a particular one; that the Society had never before in one year lost so many distinguished Fellows by death; that the respect paid to the memory of the illustrious dead might, he hoped, awaken a feeling of emulation amongst the living; and that, though he was unable to do justice to their respective merits, yet he trusted that in all he said the judgement and the feelings of the Society would be in unison with his own.

He spoke of Sir H. Englefield as an accomplished gentleman, gifted with a great variety of information, possessing a considerable knowledge of astronomy, and talents for physical researches; a clear writer; a learned antiquarian; as eminently distinguished for conversational powers; as a truly honest man, and an ornament to the class of society in which he moved.

Of Sir William Herschel Sir Humphry said, that the progress of modern astronomy was so connected with his labours that his name would live as long as that science existed. He spoke of his happy and indefatigable spirit of observation, as proved by his discoveries of a new planetary system, and of a number of satellites before unknown; of his inductive powers of reasoning, and bold imagination, as shown in his views of the system of the heavens; and of his talents for philosophical experiments, as proved by the discovery of the invisible rays in the solar spectrum. "Sir William Herschel," said the learned President, "was a man who, though raised by his own efforts, by the power of his own intellect, to so high a degree of eminence, was spoiled neither by glory nor by fortune, and retained under all circumstances the native simplicity of his mind." He dwelt at some length on his amiable character, and on the felicity of his life; remarking that he died full of years and of honours; and that when unable to labour himself, he saw a kindred disposition and powers displayed by his son.

Sir Humphry then regretted the premature death of Dr. Marcet, whom he characterized as an ingenious and accurate chemist, a learned physician, a liberal, enlightened and most amiable man. He likewise gave short characters of Dr. Vince, Plumian Professor at Cambridge; of Dr. Parry, and Sir Christopher Pegg.

In eulogizing the Foreign Members, the learned President spoke of the name of Haüy as one that would always be remembered in the history of mineralogy, in consequence of his having established what may be considered as a mathematical character in detecting mineral species. He mentioned Delambre, the learned Secretary of the Royal Academy of Sciences at Paris, with great praise, as an excellent astronomer, and candid and liberal historian of his own science, and an able observer, whose name will be for ever associated with the first accurate measurement of an arc of the meridian in France. M. Berthollet he called the patriarch of modern chemistry. He dwelt on his discoveries and labours at some length, and paid a just tribute to the candour and liberality of his mind, to his warm and zealous patronage of rising genius, and to his quiet and amiable social virtues.

Sir H. Davy then read the list of members admitted into the Society since the last anniversary, amongst whom were the Rt. Hon. Robert Peel; Mr. Dalton, of Manchester; Dr. Kidd, Professor of Chemistry at Oxford; Mr. Thomson; Mr. R. Phillips; Mr. Rennie; the Right Hon. Nic. Vansittart; and M. Decandolle on the Foreign list. He then proceeded to state the decision of the Council on the award of the medal on Sir Godfrey Copley's donation, which he stated they had this year adjudged to the Rev. William Buckland, for his paper on the Fossil Bones and Teeth found in a Cave near Kirkdale in Yorkshire. In the beginning of his discourse, the learned President said that this was the first time a paper on a subject of pure geological research had been honoured by this mark of distinction. He then entered into some general views of the progress of geology, which necessarily made but slow advances, till mineralogy, which furnished its alphabet, and chemistry and comparative anatomy its logic, had advanced to the scale of exact sciences. He said that by the zeal and accurate spirit of observation of our contemporaries, more had been effected within the last twenty years than in the whole time preceding them. He mentioned generally some of the most successful labourers in the field of research, amongst whom he said Professor Buckland was highly distinguished by his indefatigable ardour for inquiry, and by his caution and sagacity in drawing conclusions. Professor Buckland's former works had considerably contributed to elucidate and advance his favourite science; but in this paper, by his industry and happy talent for observation, *an epoch* was distinctly marked in the mineral history of the globe. The learned President then, for the purpose of illustrating the subject, gave a general view of the constitution of the known part of the surface of the globe; and stated, that though it had been suspected that the fossil remains of large animals of the Hyæna, Tiger, Elephant, Rhinoceros and Hippopotamus kind, found in our diluvian strata, had been the remains of animals who once inhabited the countries in which they were found; yet that this had never been *distinctly established* till Professor Buckland described the cave in Yorkshire, in which several generations of hyænas must have lived and died. He said that two theoretical views might be taken of the subject: one, that the animals were of a peculiar species fitted to inhabit temperate or cold climates; and the other, which he thought the most probable, that the temperature of the globe had changed. He entered into some general views on this interesting subject, and its connexion with the period when the globe was in a chaotic state; with the periods of the successive creations of living beings and with the early revolutions of the system till it had attained that degree of stability which fitted it for the habitation of man, the last of created beings.

In presenting the medal to Mr. Buckland, Sir Humphry desired him to receive it as a tribute of respect from a body which he believed to be very impartial in its decisions, and which considered the advances science had made, rather than the nation, school, or individual by which they were effected. He said he hoped his example would stimulate other members of the Society to similar inquiries and labours; for that geology was abundant in objects for research, and most worthy of being pursued, on account of its connexion with the useful arts;—from the happy views it affords of the order of nature, and the assistance it lends to true religion; and from the sublime objects it presents for speculation in the great monuments of nature, marking the revolutions of the globe.

The Society then proceeded to the election of a Council and Officers for the ensuing year; when, the List being examined, it was found that the Council consisted of the following gentlemen:

Of the Old Council.—Sir Humphry Davy, Bart.; William Thomas Brande, Esq.; Samuel Goodenough, Lord Bishop of Carlisle; Taylor Combe, Esq.; Davies Gilbert, Esq.; Charles Hatchett, Esq.; John F. W. Herschel, Esq.; Sir Everard Home, Bart.; John Pond, Esq. Astronomer Royal; William Hyde Wollaston, M.D.; Thomas Young, M.D.

Of the New Council.—Charles Babbage, Esq.; Sir Gilbert Blane, Bart.; Charles Lord Colchester; John Wilson Croker, Esq.; John Earl of Darnley; Sir H. Halford, Bart. Pr. Col. Phy.; Charles Hutton, LL.D.; Captain Henry Kater; William Hasledine Pepys, Esq.; Joseph Sabine, Esq.

The Officers.—*President.* Sir Humphry Davy, Bart.—*Treasurer.* Davies Gilbert, Esq.—*Secretaries.* William Thos. Brande and Taylor Combe, Esq.

The elections were as last year unanimous.

Dec. 5. The Croonian Lecture was continued.

Dec. 12. On Metallic Titanium; by W. H. Wollaston, M.D. V.P.R.S.—On the Difference of Structure between the Human Membrana Tympani and that of the Elephant; by Sir E. Home, Bart. V.P.R.S.

Dec. 19. On the Chinese Year; by J. F. Davis, Esq. F.R.S.—On Rocks that contain Magnesia; by Charles Daubeny, M.G.S. Professor of Chemistry, Oxford. Part only of this paper was read, and the Society adjourned to the 9th of January 1823.

ASTRONOMICAL SOCIETY OF LONDON.

Dec. 13. The following papers were read:

1. On the Measurement of Altitudes by the Barometer; by Professor Littrow.

The object of this paper was to point out a more simple process than any now in use, applicable to the service of travellers, without logarithms or voluminous tables. The author animadverted on the processes already in use, and proceeded to state that he had assumed the well known formula of Laplace as the basis of his calculations, but with a new and less complicated form of expression. From this he constructed several Tables by which the approximate heights might be immediately determined, and of these several examples were given.

2. Observations on the Comet of January 1821. By J. Nicollet of Paris.

3. On the Application of Machinery to the Computation of Mathematical Tables. By Charles Babbage, Esq. F.R.S. Secretary Astron. Soc.

This was a supplement to a former paper laid before the Society, to explain some further properties which had been developed in the use of this machinery; such as, that it was capable of taking advantage of a peculiar property of the orders of differences of the sine of an arc, so as to form tables of the sines of arcs from beginning to end, with great certainty and simplicity. The author then alluded to another class of tables dependent on laws which had never yet been reduced to analysis, but which were produced by the machinery. On examination of these, and inquiring into their solution, he succeeded in arriving at it by two different processes of great simplicity, while the analytical equations to which they could alone be reduced were of the greatest difficulty. They likewise had reference to an analytical investigation of some problems relative to the game of chess.

LINNÆAN SOCIETY.

Dec. 3d and 17th. A. B. Lambert, Esq. V. P. in the chair. On these evenings, after the conclusion of Mr. W. S. MacLeay's paper mentioned in our last number, an interesting communication was read from the Rev. Wm. Kirby, F.R. and L.S. illustrative of the preceding one, and entitled "A Description of some Insects which appear to exemplify Mr. W. S. MacLeay's Doctrine of Affinity and Analogy." It treats on some remarkable instances of animals that assume the outward semblance of one tribe, while their real characters and their habits show that they actually belong to some other: so that creatures which are placed in opposition to each other are yet connected by as it were a symbolical relationship, which has not unfrequently misled the naturalist.

The learned author, after referring to, and concurring in, the curious distinction pointed out by Mr. MacLeay, between true affinities and resemblances that are merely analogical, proceeds to exemplify his remarks in descriptions of a new genus, *Catascopus*, which might be taken for a *Notiophilus* of Leach,—of *Heteromorpha*, another new genus, which, though really a Linnæan *Carabus*, would at first sight be taken for a *Nitidula*,—and a third new genus, *Mimela*, which vastly resembles an *Areoda*. He also describes a new species of *Agrion*, which comes near to *Lestes*.

The original and interesting views of the general laws manifested in the natural distribution of organized beings, which have been lately published by Mr. W. S. MacLeay in his *Horæ Entomologicæ*, and which it is the object of the above papers to illustrate and exemplify, must claim the attention of those students who, desiring to pursue Natural History philosophically, seek to ascend from an accurate knowledge of particulars, acquired by the useful and necessary aid of artificial systems, to a discovery of that arrangement which is actually to be found in Nature. For the successive and graduated series, including innumerable modes of existence and varieties of structure, which is presented to our view, together with the general laws which prevail in the relations by which species and classes are connected in one great system, are calculated to impress the mind, and to employ our highest faculties, equally with the investigation of the structure and properties of individuals, upon an accurate acquaintance with which, however, all our other knowledge must be founded.

Mr. MacLeay remarks that those who have controverted Bonnet's doctrine of a simple series or chain of affinities, have been wrong in inferring that, because the series was found not to be simple, affinities could not be continuous, and that nature presented only unconnected groups.—Mr. MacLeay reconciles the continuity of affinities with the non-existence of a simple series, by the discovery* that two kinds of relation subsist throughout nature,—the *relation of analogy*, and the *relation of affinity*,—and that these are entirely distinct. Some glimpses of this truth occurred to Pallas, and recently to other philosophic naturalists,—Agardh, Decandolle, and lastly Fries,—in the investigation of different departments of nature. The subject is highly interesting, but the further consideration of it would exceed our present limits. As, however, the *Horæ Entomologicæ* is in the hands of but few persons, the greater part of the impression having been unfortunately destroyed in a fire, we hope to give a fuller account of it at a future time.

A second paper from Mr. Kirby was also read, being an account of a new species of Eulophus (Geoffr.) "*Eu. damicornis* aureo-viridis, abdomine nigricanti, basi maculâ pallidâ subpellucidâ." It is a parasitical insect, the larva of which is gregarious, and was found infesting a caterpillar resembling that of *Bombyx Camelina.*

The Society adjourned to January 21.

GEOLOGICAL SOCIETY OF LONDON.

Papers read at this Society during the present session:

Nov. 1, 1822. On the Geology of the Ferroe Islands; by W. C. Trevelyon, Esq.

Nov. 15. On the Geology of Hungary; by the Hon. Wm. T. H. Fox Strangways.

Dec. 6. On the Geology of Arabia and some Islands in the Persian Gulf; by J. B. Fraser, Esq.—Two Letters from W. Hamilton, Esq. His Britannic Majesty's Minister Plenipotentiary at the Court of Naples, to Dr. Granville, giving a Description of the late Eruption of Vesuvius.

Dec. 20. On the Geology of the Vicinity of Boulogne; by Dr. Fitton.—Remarks on the Order of the Strata at Stinchcombe, near Dursley, made in 1821; by George Cumberland, Esq.—Letter accompanying Fossil Bones found in the Limestone Quarries at Oreston near Plymouth; by W. Clift, Esq.—Notice on a Variety of Copper Pyrites lately found in the Consolidated Mines; by John Taylor, Esq.

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* *Horæ Entomologicæ*, published by Bagster, Paternoster-row, 1819.

ROYAL GEOLOGICAL SOCIETY OF CORNWALL.

The ninth anniversary of this Society was held at Penzance on the 11th of October. The Report of the Council states that the progress of the Institution is encouraging; and regrets the loss of the late Secretary, Dr. Forbes, who has removed from the county.

The cabinets have been enriched by many valuable donations; and in particular by a splendid series of minerals from Mount Vesuvius, for which the Society is indebted to Sir Humphry Davy. The collection of specimens in most of the departments of mineralogy is now become interesting, and, being open to public inspection, creates a growing attention to the subject, and has led to the discovery of minerals hitherto unknown in this county. Invitations to correspondence have been received from foreign institutions and men of science; which shows that this Society has already attracted notice both at home and abroad.

In the publication of the second volume of Transactions it was judged proper to confine its contents entirely to papers relative to the geology and mineralogy of Cornwall; although it occasioned the regret of thereby omitting many valuable communications. A large space will be found devoted to the detail of numerous facts and experiments on the much controverted subject, the Temperature of Mines, and also on the Phænomena of Veins, which cannot fail to attract much attention and further investigation.

Since the last anniversary a Course of Lectures on the elements of chemistry has been delivered by the Secretary, Dr. Boase, to a numerous and attentive auditory, and received with much approbation. And the Council have also the pleasure to report, that there is evidently a growing attention to scientific pursuits, which encourages them to hope that the period is not distant, when the great object of this Society will be realised by the establishment of a School of Mines in Cornwall.

Papers read since the last Report.—On the Tin-ore of Botallack and Levant. By Dr. Boase, Secretary.—A further Account of the Mineralogy and Geology of St. Just. By J. Carne, Esq.—On the Serpentine District of Cornwall. By the Rev. C. Rogers.—On the Neptunian Theory of the Formation of Veins. By H. Boase, Esq. Treasurer.—On the Noxious Gases of Mines. By Dr. Boase.—On Submarine Mines. By Joseph Carne, Esq.—On the Temperature of the Cornish Mines. By M. P. Moyle, Esq.—A proposed new Method of Drawing

Mining Maps and Sections. Communicated by Mr. Fox.—On the Temperature of Mines. By R. W. Fox, Esq.—On the Utility of a School of Mines. By Dr. Boase.—An Account of the Quantity of Tin produced in Cornwall, in the Year. By Joseph Carne, Esq.—An Account of the Produce of the Copper Mines of Cornwall, in Ore, Copper, and Money, in the Year. By Mr. Alfred Jenkyns.

Among the donations of minerals are the following:

A series of Specimens from the neighbourhood of Naples; Specimens illustrative of the Geology of the Malvern Hills; Specimens from St. Just; Specimens from the Isle of Arran and Norway; Specimens illustrative of the Lizard District; Stalactites from Bermuda; Volcanic Specimens from the Mediterranean. The Horn of an Elk and other Organic Remains from Pentuan Stream-Works.

Officers and Council for the Year.

President: Davies Gilbert, Esq. M.P. V.P.R.S. &c. &c.

Vice-Presidents: William Rashleigh, Esq.; Charles Lemon, Esq.; John Scobell, Esq.; John Paynter, Esq.

Secretary: Henry S. Boase, M. D. *Treasurer:* Henry Boase, Esq. *Librarian:* T. Barham, M.D. *Curator:* Edward C. Giddy, Esq. *Assistant Secretary:* R. Moyle, Jun. Esq.

The Council: T. Bolitho, Esq.; Joseph Carne, Esq.; Stephen Davy, Esq.; Alfred Fox, Esq.; G. D. John, Esq.; Rev. C. V. Le Grice; M. P. Moyle, Esq.; Rev. Canon Rogers; H. P. Tremenheere, Esq.; John Williams, Jun. Esq.

XCVI. *Intelligence and Miscellaneous Articles.*

ELECTRICAL SHOCKS FROM A CAT.

To the Editors of the Philosophical Magazine.

Gentlemen,—Every child knows that in frosty weather the back of a cat, when rubbed, possesses the power of emitting *electrical sparks;* but as I am not aware of its being generally known that an *electrical shock* may be taken from the animal, I have not thought the fact too trifling to merit your notice.

A few days ago, when sitting in a warm room before a good fire, a favourite cat unceremoniously placed himself upon my lap. After amusing myself for a short time in exciting the sparks from his back, I was, in consequence of feeling a disagreeable starting sensation in the left hand, about to place the animal on the carpet, when it occurred to me that the effect might be *electrical,* as my left hand had been placed under his throat, the middle finger and the thumb gently pressing the bones of his shoulders. while my right was passing along his back. Replacing the animal in his situation, I carefully repeated the action, and found my hypothesis to be correct; as I received at short intervals a disagreeable sensation in the left hand and arm, exactly resembling a slight shock from the Leyden phial, the body of the animal with my arms and back forming the electric circle. Afterwards, changing the position of the creature and placing my right hand under his throat, while I passed the left along his back, I experienced a similar sensation in the *right* hand. On trial, some friends who were present found the same result. I have repeated the experiment, and, the present state of the atmosphere being favourable, always with the same result—provided the cat had lain some time before the fire.

If the object of this trifling communication be not generally known, perhaps the publishing of it will serve to amuse your readers, and add to our stock of philosophical *curiosities.*

I remain, gentlemen,
Your obedient servant,

Dec. 21, 1822. JOHN GLOVER.

P. S. It is not indispensably necessary that the finger and thumb be placed on the shoulders: the bone of the jaw has the same effect.

ERROR IN TABLES OF ABERRATION, ETC.

☞ We are requested by Mr. F. Baily to correct an error which appears in No. 294 of our Journal, in the account of the *New Tables of Aberration and Nutation.* It was there stated that the *whole* of the tables were computed by MM. Rosenbergh and Scherke; whereas it was only the table containing the values of C and D that was calculated by those gentlemen. The table of the constant quantities (denoted by *a*, *b*, *c*, *d*, and *a'*, *b'*, *c'*, *d'*,) were computed by Dr. Ursin and Mr. Hansen, under the immediate direction of the celebrated M. Schumacher, so well known for his zeal and abilities in the cause of science, and equally distinguished for his liberal views in promoting and widely extending the diffusion of knowledge.

NATURAL HISTORY.—SNAKE.

"*Demerara, 24th September.* Yesterday a Carnody snake was killed on Plantation Huis l'Dieron, on the west coast of Essequibo, measuring 14 feet long, and 11 inches in circumference at the natural size of the stomach; but the stomach was distended to the enormous size of 31 inches. On opening it, it was found to contain an entire alligator recently swallowed (decomposition having scarcely commenced), and measuring six feet long by 28 inches circumference. From the appearance of the neck of the alligator, it is evident that the snake destroyed him by entwining round that part; and so severe seemed to be the constriction, that the eyes of the alligator were actually started from their sockets."

RUSSIAN VOYAGE OF DISCOVERY.

The ships Golownin and Baranow, sent in 1821 by the Russian American Company to make discoveries on the north-west coast of America, have returned safe. Besides making a more accurate survey of the north-west coast, they have discovered a pretty large island, called Numirak, situated, according to their account, in 59° 54′ 57″ north latitude, and 193° 17′ 12″ east longitude.

FIRE IN A COAL-PIT.—DIRECTION OF CURRENTS.

AYR, Nov. 21.—On the night of Saturday last, a large fire, termed by the colliers a lamp, was lighted in the Sutfield Coal-pit, for the purpose of carrying off the air in a certain direction. In the course of the night, and after the workmen had left the pit, the current of air changed its course; in consequence of which the flames of the lamp communicated with a quantity of timbers, which was discovered upon Sunday morning to be on fire, and burning with great violence. The fire-engines from the town were conveyed to the spot, the pipes of which were introduced into the pit; and although the supply of water was plentiful, yet their united streams had no effect in quenching the flames. It was next deemed advisable to exclude the air, by covering up the mouth of the pit; and in this state it remained until yesterday, when the smoke having apparently subsided, the covering was removed, and several men went down, who report little damage to be done beneath, with the exception of the loss of ten ponies which were suffocated by the smoke. The fire was found to be nearly exhausted, and it is expected that the pit will be in working order in a few days.

NOTE. In mines which have a number of pits, frequent changes in the direction of the currents of air may be observed, such changes being generally produced by alterations in the weather.

Dr. Forbes in his paper on the Temperature of Mines in the Transactions of the Royal Geological Society of Cornwall, vol. ii. p. 168, says, "The cooling effect of high winds is very perceptible even at the bottom of shallow mines, and it appears that the currents of air in the very deepest mines are considerably influenced by their force and direction. The following observations on this subject made in Dolcoath Mine were communicated to

Dr. Forbes by Mr. John Rule, jun., one of the superintendants. "I have made some experiments to ascertain the direction of the currents of air in this mine, and find that in 25 of our principal shafts, 13 have a strong current downwards, and 12 about the same degree of current upwards; they differ, however, with respect to the strength of the currents, some being very strong, others less so." "I am enabled to state that the currents vary as the wind does, so that those shafts which sometimes have a current downwards, on a change of the wind have an upward current, and *vice versâ*. The same thing takes place with regard to the levels under ground—changes of wind making the current of air run in opposite directions at different times, and altering its force, so that when it blows hard the current is strong underground, and *vice versâ*."

These observations agree with our own experience, and that of most practical men: it is not to be wondered at, therefore, that such an accident should happen as that detailed above: it is rather, indeed, extraordinary, as fire is often used for ventilating mines, when explosive gases are present, that some accidents have not occurred of a more serious nature. The only effectual remedy for foul air in mines is drawing it off by *mechanical means*, a process which has been tried in some cases with success, has been much praised and talked of, but we believe very little practised.—*J. F.*

LIST OF PATENTS FOR NEW INVENTIONS.

To Joseph Egg, of Piccadilly, in the parish of St. James, Westminster, gun-maker, for certain improvements in the construction of guns and fire-arms upon the self-priming and detonating principle.—Dated 26th November 1822.—2 months allowed to enrol specification.

To Thomas Ibbotson, of Sheffield, Yorkshire, fender manufacturer, for his improved fender, capable of being extended or contracted in length so as to fit fire-places of different dimensions.—28th November.—2 months.

To John Dixon, of Wolverhampton, Staffordshire, brass-founder, for certain improvements on cocks such as are used for drawing off liquids.—28th November.—2 months.

To Joseph Woollams, of the city of Wells, Somersetshire, land-agent, for certain improvements in wheeled carriages of various descriptions, to counteract the falling and facilitate the labour of animals attached to them, and to render persons and property in and near them more secure from injury.—5th December.—6 months.

To William Robson, of St. Dunstan's Hill, Tower-street, London, printer and stationer, for his method to prevent or protect against fraudulent practices, upon bankers' checks, bills of exchange, and various species of mercantile, commercial, and other correspondence.—10th December.—6 months.

To Jacob Perkins, late of Philadelphia, in the United States of America, but now of Fleet-street, London, engineer, who, in consequence of communications made to him by a certain foreigner residing abroad, and discoveries by himself, is in possession of certain improvements in steam-engines.—10th December.—6 months.

To Samuel Parker the younger, of Argyl-street, in the parish of St. James, Westminster, Middlesex, bronzist, for certain improvements in the construction of lamps.—10th Dec.—2 m.

To William Bundy, of Fulham, Middlesex, mathematical instrument maker, for his machine for breaking, cleaning and preparing flax, hemp, and other vegetable substances containing fibre.—16th December.—6 months.

To Thomas Barnard Williamson Dudley, of King-street, in the parish of St. Ann, Westminster, Middlesex, mechanist, for his method of making or manufacturing malleable cast-metal shoes for draft and riding horses, and other animals, upon a new and improved plan or principle.—16th Dec.—6 m.

To John Nicholson, of Brook-street, Lambeth, Surry, civil engineer, for certain apparatus for the more conveniently applying heat to certain instruments of domestic use.—16th December.—6 months.

To John Dumbell, of Howley House, Warrington, Lancashire, merchant, for certain improvements relative to carriages, which may be applied thereto, or in improving of the organization, driving, actuating, accelerating, or moving of vehicles and carriages in general.—16th December.—6 months.

To John Bainbridge, of Bread-street, Cheapside, London, merchant, who, in consequence of a communication made to him by Amos Thayer, junior, of the city of Albany, in the United States of America, mechanist, is in possession of certain improvements on rotatory steam-engines.—16th Dec.—6 mon.

To Matthias Wilks, of Dartford, Kent, seed-crusher, for his method of refining oil produced from seed.—20th December.—6 months.

To Thomas Linley, of Sheffield, Yorkshire, bellows-maker, for his method of increasing the force or power of bellows.—20th December.—2 months.

To Sir James Jelf, of Oaklands, near Newnham, Gloucestershire, knight, for his combination of machinery for working and ornamenting marble and other stone for jambs, mantles, chimney-pieces, and other purposes.—20th December.—6 mo.

To John Isaac Hawkins, of Pentonville, civil engineer, and Sampson Mordan, of Union-street, City-road, portable pen-maker, for their improvements on pencil holders or port crayons, and on pens for the purpose of facilitating writing and drawing, by rendering the frequent cutting or mending of the points or nibs unnecessary.—20th December.—6 months.

To William Pass, of Curtain-road, in the parish of St. Leonard, Shoreditch, Middlesex, dyer, for his improvement in calcining and smelting of various descriptions of ore.—20th December.—6 months.

METEOROLOGICAL TABLE.

The *London* Observations by Mr. CARY, of the Strand.

The *Boston* Observations by Mr. SAMUEL VEALL.

Days of Month.	Thermometer.				Height of the Barom. Inches.		Weather.	
	London.			Boston.				
1822.	8 A. M.	Noon.	11 P. M.		Lond.	Boston	London.	Boston.
Nov. 27	47	52	44	48	29·65	29·26	Fair	Fine
● 28	46	46	40	41·5	·43	29·13	Rain	Cloudy, Rain A. M.
29	40	40	36	40·5	·30	29·13	Showery	Do. Frosty morning
30	32	42	38	41·5	·41	29·05	Showery	Rain
Dec. 1	41	47	46	40·5	·24	28·82	Rain	Rain
2	35	42	42	40	·10	28·92	Fair	Fine
3	40	42	32	36·5	·40	29·25	Fair	Fine
4	37	47	38	43	·54	29·22	Fair	Rain [at night
5	35	42	32	39	[illegible]0	29·50	Fair	Fine, Wind and Rain
6	40	45	37	42·5	[illegible]4	29·[illegible]	Fair	Fine, tremendous
7	36	43	35	43	30[illegible]	[illegible]9·75	Fair	Fine [gale A.M.
8	32	42	[illegible]	[illegible]5	·27	30·05	Fair	Fine
9	46	46	47	48	·14	29·80	Rain	Rain
10	40	44	35	43·5	·45	30·26	Fair	Fine [Frost
11	32	32	34	30	·60	30·45	Foggy	Cloudy, sharp Rime
12	33	42	36	39	·60	30·42	Fair	Fine
☾ 13	33	40	34	38·5	·37	30·22	Cloudy	Cloudy
14	33	38	33	36·5	·25	30·10	Cloudy	Cloudy
15	33	36	33	31·5	·05	30·	Cloudy	Fine
16	32	33	32	32	·20	30·15	Cloudy	Cloudy
17	31	34	37	33·5	·38	30·15	Cloudy	Cloudy
18	42	47	42	46·5	·25	30·	Cloudy	Rain
19	40	36	30	40	·27	30·20	Cloudy	Cloudy
20	30	33	30	35	·3[illegible]	30·25	Fair	Fine
21	29	32	29	34	·32	30·25	Fair	Fine
22	29	38	36	37	·15	30·12	Fair	Fine
23	37	42	40	43·5	·12	29·97	Cloudy	Fine
24	40	35	35	36	·13	30·	Cloudy	Cloudy
25	35	35	30	36	·45	30·43	Cloudy	Cloudy
26	29	35	29	36	·44	30 35	Fair	Fine

The quantity of Rain in London since the 26th Nov. amounts to 2 In. 3 tenths.

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